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MEDICAL ESSAYS

AND

OBSERVATIONS.

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MEDICAL ESSAYS

OBSERVATIONS

SOCIETY OF EDINBURGH



VOLUME III

The First Edition

EDINBURGH

JOHN BALFOUR

MDCCLXXI

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M E D I C A L
E S S A Y S
A N D
O B S E R V A T I O N S.

A R T I C L E I.

The Meteorological Register.

THE instruments described in Art. II. of our first volume, were employed in making the following Observations.

VOL. III.

A

JUNE

2 MEDICAL ESSAYS

JUNE 1733.

D.	Hour.	Baro.		Ther.		Hyg.		Wind.		Weather.	Rain.	
		In.	D.	In.	D.	I.	D.	Dir.	For.		In	D.
29	a m	19	9	12	7	0	9	N		2	fair	
	8 p m	19	8	13	3	0	9	W		3	fair	
29	a m	19	8	13	7	1	1	W by N		2	fair	
	8 p m	19	9	14	2	1	5	N by W		1	fair	
30	a m	30	0	12	6	1	3	E		1	cloudy	
	7 p m	30	0	13	4	1	1	E		1	cloudy	
30	a m	30	0	13	6	1	1	S E		1	cloudy	
	8 p m	19	9	13	0	1	5	E		0	cloudy	
31	a m	19	8	13	3	1	8	E		2	cloudy	
	7 p m	19	8	12	6	1	5	E		1	cloudy	
6	a m	19	8	13	4	1	5	E		1	fair	
	7 p m	19	8	12	8	1	5	E		2	fair	
7	a m	19	8	13	4	1	5	E		1	fair	
	7 p m	19	8	15	0	1	2	E		1	cloudy	
8	a m	19	7	12	9	1	5	E		2	cloudy	
	6 p m	19	8	13	0	1	3	E		2	fair	
9	a m	19	7	13	3	1	2	E		1	fair	
	7 p m	19	6	13	2	1	4	S E		1	cloudy	
10	a m	19	8	12	7	1	2	N E		1	fair	
	7 p m	19	9	12	7	1	2	E		2	cloudy	
11	a m	19	9	13	8	1	4	E		1	fair	
	7 p m	19	9	13	7	1	3	E		c	cloudy	
12	a m	19	9	14	8	1	5	S E		c	cloudy	0,034
	8 p m	19	7	13	5	1	8	E		2	rain	
13	a m	19	7	13	4	2	8	N		c	rain	
	5 p m	19	7	14	5	2	1	N		1	cloudy	
14	a m	19	7	14	4	2	1	N		1	fair	0,386
	8 p m	19	7	15	3	1	4	W		1	fair	
15	a m	19	8	15	6	1	4	S W		1	cloudy	0,350
	8 p m	19	8	15	5	1	1	S W		1	cloudy	
16	a m	19	7	16	0	1	5	S		c	cloudy	0,190
	8 p m	19	6	14	3	1	4	S W		2	fair	
											0,960	

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AND OBSERVATIONS

JUNE 1733.

D. Hour.	Baro. In D.	Ther. Hyg. In D. I. D.			Wind. Dir. For.	Weather.	Rain.
17 9 a m	29 6	5	6	1	2 S	2 cloudy	0,960
8 p m	29 5	14	3	1	4 S W	2 cloudy	0,085
18 9 a m	29 5	14	6	1	6 W	2 cloudy	0,055
8 p m	29 3	13	5	1	4 W	2 cloudy	
19 9 a m	29 2	14	1	1	4 W	2 fair	0,035
9 p m	29 1	13	5	1	5 S W	1 rain	
20 9 a m	28 9	14	1	1	8 S E	1 rain	0,316
5 p m	28 9	14	5	1	5 W	2 fair	
21 9 a m	29 3	13	9	1	7 W	3 fair	0,173
5 p m	29 6	13	7	1	3 W	3 fair	
22 9 a m	29 8	14	2	1	4 S W	1 fair	0,145
5 p m	29 9	14	6	1	3 S W	1 cloudy	
23 9 a m	29 9	15	0	1	5 W	2 cloudy	
8 p m	29 9	15	2	1	2 W	0 fair	
24 9 a m	29 9	14	9	1	3 E	1 fair	
8 p m	29 9	15	4	1	5 E	0 fair	
25 9 a m	29 8	16	0	1	4 S W	0 cloudy	0,084
9 p m	29 9	16	0	1	3 W	0 fair	
26 9 a m	29 9	15	3	1	7 E	0 fog	0,100
7 p m	29 9	16	1	1	7 E	0 fair	
27 9 a m	29 9	15	2	1	8 E	1 fog	
8 p m	29 8	14	1	2	5 E	2 rain	
28 9 a m	29 8	16	0	1	6 S W	2 cloudy	0,150
7 p m	29 8	15	0	1	2 S W	1 cloudy	
29 9 a m	29 8	14	8	1	4 S W	2 fair	
8 p m	29 8	14	1	1	7 S W	2 cloudy	
30 9 a m	29 9	14	7	1	6 S W	2 fair	0,035
8 p m	29 9	14	7	1	6 S W	2 fair	
Total depth 2,138							
H. at a med.	29 8	14	2	1	3		
Gr. height	30 0	16	1	2	8		
L. height	28 9	12	6	0	9		

MEDICAL ESSAYS

JULY 1733.

D.	Hour.	Baro. In. D.	Ther. In D.	Hyg. f. D.	Wind. Dir. For.	Weather.	Rain. In. D.
1	9 a m	29	915	51	4 N W	1 fair	
	8 p m	29	915	01	4 W	1 fair	
2	9 a m	29	915	31	5 E	1 fair	
	8 p m	29	915	81	3 E	1 fair	
3	9 a m	29	914	51	4 E	1 fair	
	7 p m	30	015	01	5 E	1 fair	
4	9 a m	30	114	71	9 E	1 fair	
	5 p m	30	015	11	7 E	1 fair	
5	9 a m	29	915	71	7 S E	1 fair	
	5 p m	29	915	61	6 W	2 cloudy	0,045
6	9 a m	30	015	01	5 S E	2 fair	
	5 p m	30	015	71	5 S W	2 fair	
7	8 a m	30	114	51	5 W	2 cloudy	
	9 p m	30	015	61	6 W	0 fair	
8	9 a m	29	915	11	4 W	3 fair	
	8 p m	29	914	21	4 W	2 cloudy	
9	9 a m	29	714	31	6 W	3 cloudy	
	5 p m	29	714	31	6 W	3 cloudy	
10	9 a m	29	713	81	3 N W	3 fair	
	8 p m	29	813	51	2 N W	3 cloudy	0 145
11	9 a m	29	814	21	5 W	3 fair	
	5 p m	29	814	91	5 W	3 cloudy	
12	9 a m	29	813	91	6 W	2 cloudy	
	5 p m	29	715	01	6 W	1 cloudy	
13	9 a m	29	614	51	7 W	2 fair	
	5 p m	29	515	31	4 W	3 fair	
14	9 a m	29	612	21	1 N E	2 cloudy	0,093
	7 p m	29	613	21	4 N E	1 fair	
15	9 a m	29	614	61	4 N E	1 fair	
	8 p m	29	613	71	3 N E	1 fair	
16	8 a m	29	613	12	2 E	2 rain	
	5 p m	29	612	71	8 N E	2 cloudy	

0,63

AND OBSERVATIONS. 5

JULY 1733.

Rain. In. D.	D. Hour.	Baro. In D.	Ther. In D.	Hyg I. D.	Wind. Dir. For.	Weather.	Rain.
	17 8 a m	29 6 13	43 5	NE	2	cloudy	0,283
	4 p m	29 6 14	02 0	NE	1	cloudy	
	18 9 a m	29 7 14	12 0	N	1	fair	0,054
	9 p m	29 7 14	01 6	NE	0	fair	
	19 8 a m	29 7 13	61 6	N W	2	fair	
	8 p m	29 7 14	11 7	N W	2	fair	
	20 9 a m	29 8 14	01 6	W	2	fair	
	5 p m	29 8 15	41 4	W	2	fair	
	21 8 a m	29 7 14	02 0	W	2	fair	
	7 p m	29 7 15	61 2	N W	1	fair	
0,045	22 9 a m	29 8 15	51 5	W	2	fair	
	6 p m	29 8 15	51 3	W	2	fair	
	23 9 a m	29 8 14	91 6	W	2	cloudy	
	5 p m	29 8 15	81 6	W	3	cloudy	
	24 9 a m	29 8 15	12 0	W	2	fair	
	5 p m	29 8 15	31 4	W	2	fair	
	25 8 a m	29 9 14	01 7	W	2	fair	0,115
	5 p m	29 9 14	81 3	W	3	fair	
	26 9 a m	29 8 14	41 5	W	3	fair	
0 145	8 p m	29 8 14	01 5	W	2	fair	
	27 8 a m	29 8 13	31 6	W	2	fair	
	5 p m	29 8 14	81 0	W	2	fair	
	28 9 a m	29 7 14	71 4	S	1	fog	
	4 p m	29 6 15	31 3	S by W	0	cloudy	
	29 9 a m	29 5 14	31 5	E	1	fair	
	7 p m	29 4 14	31 9	W	1	cloudy	
0,093	30 8 a m	29 4 13	82 0	N by W	1	fair	0,186
	5 p m	29 4 14	51 1	N W	2	fair	
	31 8 a m	29 4 13	01 7	W	2	fair	
	7 p m	29 4 13	21 4	N W	2	fair	
H at a med. 29		7 14	6 1	6	Total depth 0,638		
Gr. height 30		1 15	8 3	5			
L. height 29		4 12	2 1	0			

6 MEDICAL ESSAYS

AUGUST 1733.

D.	Hour.	Baro. In. D.	Ther. In. D.	Hyg. I. D.	Wind. Dir For.	Weather.	Rain. In D.
19	a m 29	5 13	5 1	3	W	2 cloudy	
6	p m 29	6 13	9 1	3	N W	1 fair	
29	a m 29	6 12	7 1	5	W	1 fair	
8	p m 29	6 13	8 1	0	E	1 fair	
39	a m 29	6 13	6 1	3	E	1 fair	
8	p m 29	5 13	5 1	4	E	1 fair	
49	a m 29	4 13	3 1	5	S E	2 cloudy	
8	p m 29	5 13	6 1	5	E	1 cloudy	
59	a m 29	5 13	8 1	8	E	1 cloudy	
8	p m 29	5 13	5 1	8	E	1 rain	
69	a m 29	4 14	4 2	4	E	1 rain	0.056
8	p m 29	4 13	3 3	7	E	1 fog	
79	a m 29	4 13	7 3	1	E	2 cloudy	0.093
7	p m 29	4 13	0 3	2	N E	1 fog	
89	a m 29	4 13	6 3	3	N E	1 cloudy	
8	p m 29	5 13	6 2	2	W	1 cloudy	
99	a m 29	7 13	1 1	9	N W	0 cloudy	
5	p m 29	7 14	8 1	5	N W	0 fair	
109	a m 29	7 13	6 1	8	S W	0 cloudy	0.075
7	p m 29	7 13	4 1	9	S E	1 cloudy	
118	a m 29	7 12	4 2	0	W	1 fair	
6	p m 29	8 14	5 1	3	W	1 fair	
129	a m 29	7 15	0 1	4	S W	4 cloudy	0.035
6	p m 29	7 13	5 1	3	S W	4 cloudy	
138	a m 29	7 13	1 1	4	W	2 fair	
5	p m 29	8 14	3 1	2	W	1 fair	
149	a m 29	4 14	1 1	3	S W	3 fair	0.189
4	p m 29	4 13	8 1	4	W	3 fair	
159	a m 29	7 12	4 1	7	W	1 fair	0.210
5	p m 29	7 14	0 1	4	W	1 cloudy	
169	a m 29	6 12	0 1	8	N W	1 rain	0.490
4	p m 29	7 13	0 1	7	N W	0 cloudy	

1.147

AND OBSERVATIONS. 7

AUGUST 1733.

Rain. In D.	D.	Hour.	Baro. In D.	Ther. In D.	Hyg. I. D.	Wind. Dir. For.	Weather.	Rain.
	17	9 a m	29	7 12	4 1	6 S W	2 fair	1,147
		5 p m	29	4 13	2 1	6 S W	4 cloudy	0,365
	18	9 a m	29	3 13	6 2	0 S W	3 cloudy	
		5 p m	29	2 13	4 1	8 S W	3 cloudy	
	19	9 a m	29	7 12	5 1	5 N W	2 cloudy	0,154
		7 p m	29	8 13	0 1	3 N W	0 cloudy	
	20	8 a m	29	7 13	8 1	7 S W	0 rain	
		5 p m	29	6 15	0 1	9 S W	1 cloudy	
	21	8 a m	29	6 14	1 2	3 S W	2 cloudy	
		7 p m	29	6 12	9 1	9 W	1 cloudy	
0,056	22	8 a m	29	7 12	6 1	9 W	1 fair	
		7 p m	29	7 12	9 1	5 W	1 fair	
0,093	23	9 a m	29	8 12	7 1	8 W	1 fair	0,193
		7 p m	29	8 13	2 1	4 W	1 fair	
	24	9 a m	29	7 13	2 1	4 S W	2 cloudy	0,055
		6 p m	29	5 13	9 1	5 S W	3 fair	
	25	9 a m	29	5 12	3 1	5 W	2 fair	
		7 p m	29	6 12	3 1	5 W	2 fair	
0,075	26	9 a m	29	6 12	9 1	6 N W	2 fair	
		6 p m	29	7 13	2 1	2 N W	1 fair	
	27	9 a m	29	6 12	8 1	9 S W	1 cloudy	0,100
		7 p m	29	6 12	8 1	6 W	1 fair	
0,035	28	9 a m	29	5 11	5 1	6 W	1 fair	
		6 p m	29	4 12	7 1	6 S	0 cloudy	
	29	9 a m	29	1 12	1 1	9 S W	1 rain	0,286
		5 p m	29	1 12	6 1	4 W	1 fair	0,375
0,188	30	9 a m	29	4 12	5 1	5 N W	2 fair	
		5 p m	29	5 13	0 1	3 N W	1 fair	
0,210	31	9 a m	29	3 12	1 1	9 S	1 rain	
		6 p m	29	2 12	4 1	4 W	2 fair	
0,490								Total depth 2,675
1,147	H. at med. 29		6 13	2 1	7			
	Gr. height 29		8 15	0 3	3			
	L. height 29		1 12	1 1	0			

8 MEDICAL ESSAYS

SEPTEMBER 1733.

D.	Hour.	Baro. In D.	Ther. In D.	Hyg. I. D.	Wind. Dir. For.	Weather.	Rain. In D.
19	a m 29	2 12	6 1	7	S W	2 fair	0,084
	6 p m 29	0 11	5 1	5	W	2 cloudy	
29	a m 28	9 12	7 1	5	S W	1 cloudy	0,055
	6 p m 29	0 12	6 1	6	E	1 fair	
38	a m 29	2 12	0 1	7	N W	2 cloudy	
	4 p m 29	4 12	0 1	2	N W	2 fair	
49	a m 29	5 12	0 1	7	S W	3 fair	0,130
	4 p m 29	2 11	5 1	5	S W	2 rain	
59	a m 29	0 12	2 1	8	S W	3 fair	0,067
	5 p m 29	3 12	4 1	5	W	3 fair	
68	a m 29	6 11	3 2	0	W	2 fair	
	6 p m 29	7 12	3 1	4	S W	2 fair	
78	a m 29	6 11	4 1	6	W	2 fair	
	5 p m 29	7 12	1 1	5	W	2 fair	
89	a m 29	8 11	0 1	9	W	2 fair	0,108
	6 p m 29	9 11	0 1	5	N	2 cloudy	
99	a m 30	0 10	5 1	6	N	2 fair	
	5 p m 30	1 12	1 1	3	N by W	1 fair	
10	a m 30	1 11	3 1	5	S W	1 fair	
	5 p m 30	1 12	3 1	3	S by E	1 fair	
11	a m 30	1 11	3 1	5	S by E	2 fair	
	4 p m 30	0 13	5 1	2	S by E	2 fair	
12	a m 29	8 11	9 1	7	S E	2 fair	0,077
	5 p m 29	7 13	4 1	6	S E	2 rain	
13	a m 29	8 13	3 1	9	S	1 rain	0,030
	5 p m 29	8 14	3 1	8	S	1 cloudy	
14	a m 29	9 13	0 3	0	S E	1 mist	0,006
	5 p m 29	9 13	4 2	8	S E	1 mist	
15	a m 30	0 12	5 3	3	S E	1 mist	
	5 p m 30	0 12	3 3	2	E	2 mist	
16	a m 30	0 12	2 2	7	E	2 fog	
	5 p m 30	0 11	3 2	1	E	1 fair	

0,677

D. F.
179
5
188
5
199
5
209
4
219
6
229
5
239
4
249
5
259
5
269
5
279
4
289
5
299
5
309
5
H.at
Gr.
L. 1

AND OBSERVATIONS. 9

SEPTEMBER 1733.

Rain. In D.	D.	Hour.	Baro. In D.	Ther. In D.	Hyg. In D.	Wind. Dir. For.	Weather.	Rain.
0,084	17	9 a m 30	1 12	0 2	1	E	1 fog]	0,677
0,055		5 p m 30	1 13	0 1	9	E	1 cloudy	
	18	8 a m 30	2 12	3 2	1	S W	1 cloudy]	
		5 p m 30	2 13	0 2	1	S W	1 cloudy	
0,239	19	9 a m 30	2 13	2 2	4	S W	2 fair	
		5 p m 30	1 13	8 1	7	W	2 fair	
0,067	20	9 a m 29	9 13	6 1	9	S W	2 cloudy	
		4 p m 29	9 13	8 1	0	S W	2 rain	
	21	9 a m 29	8 11	9 2	2	W by S	2 fair	0,356
		6 p m 29	9 11	7 2	0	S W	2 fair	
	22	9 a m 29	9 12	2 2	5	W	2 fair	
		5 p m 29	9 12	8 1	9	W	1 fair	
0,108	23	9 a m 29	8 12	5 2	0	S W	1 fair	0,240
		4 p m 29	7 13	1 1	6	S W	1 fair	
	24	9 a m 29	4 12	5 2	2	S W	2 fair	
		5 p m 29	3 13	2 1	7	S	2 lowering	
	25	9 a m 29	1 11	0 1	9	S W	2 fair	
		5 p m 28	9 11	8 1	8	S by W	2 rain	
	26	9 a m 28	4 10	8 2	0	S W	4 cloudy	0,395
		5 p m 28	6 10	5 1	7	S W	3 cloudy	
0,077	27	9 a m 28	9 11	1 1	8	W	3 fair	0,094
		4 p m 29	2 11	8 1	5	W	3 fair	
0,030	28	9 a m 29	1 12	3 2	1	S W	2 cloudy	
		5 p m 29	1 12	3 1	7	S W	4 cloudy	
0,026	29	9 a m 29	4 11	7 1	8	W	4 cloudy	
		5 p m 29	5 11	6 1	7	W	3 fair	0,073
	30	9 a m 29	7 11	4 1	9	W	2 fair	
		5 p m 29	7 11	1 1	7	W	1 cloudy	
	H. at med. 29		6 12	2 1	9	Total depth 1,825		
	Gr. height 30		2 13	8 3	3			
0,677	L. height 28		4 10	5 1	2			

10 MEDICAL ESSAYS

OCTOBER 1733.

D.	Hour.	Baro. In. D.	Ther. In. D.	Hyg. I. D.	Wind. Dir. For.	Weather.	Rain. In. D.
19	a m	29	8 11	2 1	5 S W	1 rain	0,045
5	p m	29	5 12	4 1	9 S W	1 cloudy	
29	a m	29	5 13	3 2	1 S W	1 cloudy	
5	p m	29	4 13	7 1	8 S W	3 cloudy	
39	a m	29	1 12	9 1	7 S W	4 cloudy	0,060
5	p m	29	1 12	5 1	7 S W	4 fair	
49	a m	29	5 11	6 2	0 W	2 fair	
5	p m	29	7 11	8 1	5 N W	2 fair	
59	a m	30	1 10	6 1	3 W by N	1 fair	
4	p m	30	2 11	0 1	4 W by N	1 fair	
69	a m	30	3 9	9 1	7 N W	1 fair	
5	p m	30	4 10	4 1	7 N W	0 fair	
79	a m	30	4 10	5 1	8 S W	1 cloudy	
4	p m	30	2 11	3 1	7 S W	1 cloudy	
89	a m	30	0 11	4 1	7 S W	1 fair	
5	p m	30	0 11	7 2	7 W by S	2 cloudy	
99	a m	30	0 11	4 2	6 N E	2 rain	
5	p m	30	0 10	9 2	7 E	2 fog	
109	a m	29	9 11	1 2	4 S E	1 cloudy	0,124
5	p m	29	8 11	4 2	2 S E	2 cloudy	
119	a m	29	7 11	3 2	3 S E	1 cloudy	
5	p m	29	6 11	3 2	0 E	1 cloudy	
129	a m	29	5 11	2 2	4 S E	1 lowring	
5	p m	29	4 11	3 2	3 E	1 cloudy	
139	a m	29	4 9	1 2	2 N W	2 fair	
5	p m	29	5 10	7 1	1 N W	3 cloudy	
149	a m	29	7 10	2 1	9 N W	2 fair	
5	p m	29	8 10	4 1	5 N W	2 fair	
159	a m	30	0 9	4 1	7 N W	2 fair	
5	p m	30	0 10	2 1	7 N W	2 fair	
169	a m	30	1 10	1 2	0 N W	1 fair	
5	p m	30	2 10	8 1	7 N W	1 fair	

0,120

H. at a m
Gr. height
L. height

AND OBSERVATIONS. 11

OCTOBER 1733.

Rain. In. D.	D. Hour.	Baro. In. D.	Ther. In. D.	Hyg. I. D.	Wind. Dir. For.	Weather.	Rain.
0,045	17 9 a m	30	3 8	9 1	9 W by N	1 fair	0,129
	5 p m	30	3 9	4 1	7 W by N	1 fair	
	18 9 a m	30	4 9	9 1	9 S W	1 fair	
	4 p m	30	4 11	0 1	8 S	1 fair	
0,066	19 9 a m	30	4 10	8 1	9 S W	1 fair	
	4 p m	30	4 11	5 1	8 W	0 fair	
	20 9 a m	30	3 10	0 1	8 W	2 fair	0,036
	4 p m	30	3 11	0 1	7 W	2 fair	
	21 9 a m	30	3 10	6 1	7 W	0 cloudy	
	4 p m	30	2 11	1 1	7 W	2 cloudy	
	22 9 a m	30	1 10	7 2	0 S W	2 fair	
	5 p m	30	1 10	8 2	0 W	2 fair	
	23 9 a m	30	0 10	6 2	0 W	0 cloudy	
	4 p m	29	9 11	0 2	0 W	0 cloudy	
	24 9 a m	29	7 10	1 2	0 S W	0 fair	
	4 p m	29	5 10	8 1	7 S W	2 cloudy	
	25 9 a m	29	0 11	5 2	2 S W	2 cloudy	
	4 p m	28	9 11	9 1	7 S W	2 cloudy	
0,124	26 9 a m	29	0 10	8 1	9 S W	3 fair	
	4 p m	29	0 11	2 2	0 S W	3 cloudy	0,206
	27 9 a m	29	1 10	1 1	9 S W	3 cloudy	
	4 p m	29	2 9	8 1	8 S W	3 rain	0,154
	8 9 a m	29	4 10	3 2	0 S W	2 cloudy	
	4 p m	29	3 11	9 2	4 S W	3 rain	0,380
	29 9 a m	29	1 11	7 2	4 S-W	4 cloudy	
	4 p m	29	1 10	6 1	9 W	4 cloudy	0,084
	30 9 a m	29	7 9	0 1	6 W	2 cloudy	
	4 p m	30	0 9	2 1	4 N W	2 cloudy	
	31 9 a m	30	2 9	3 1	5 S W	2 cloudy	
	5 p m	29	9 1	3 1	9 S W	3 cloudy	
	H. atamed.	29	8 10	9 1	9	Total depth 1,083	
0,229	Gr. height	30	4 13	7 2	7		
	L. height	28	9 8	9 1	3		

12 MEDICAL ESSAYS

NOVEMBER 1733.

D.	Hour.	Baro. In. D.	Ther. In. D.	Hyg. I. D.	Wind. Dir. For.	Weather.	Rain. In. D.
1	9 a m	29	71	2	3	SW	3 cloudy
	4 p m	29	71	2	3	SW	2 cloudy
2	9 a m	29	71	2	3	W	3 fair
	5 p m	29	71	2	3	W	3 cloudy
3	9 a m	29	71	2	3	W	2 cloudy
	4 p m	29	71	2	3	W	2 fair
4	9 a m	29	71	2	3	N	1 fair
	4 p m	29	71	2	3	N	3 cloudy
5	9 a m	29	71	2	3	N	2 cloudy
	4 p m	29	71	2	3	W	0 cloudy
6	9 a m	29	71	2	3	W	1 cloudy
	4 p m	29	71	2	3	W	1 cloudy
7	9 a m	29	71	2	3	W	1 cloudy
	5 p m	29	71	2	3	W	1 cloudy
8	9 a m	29	71	2	3	S	0 cloudy
	5 p m	29	71	2	3	S	3 fair
9	9 a m	29	71	2	3	SW	2 fair
	4 p m	29	71	2	3	SW	3 fair
10	9 a m	29	71	2	3	SW	4 cloudy
	4 p m	29	71	2	3	W	2 fair
11	9 a m	29	71	2	3	W	2 cloudy
	4 p m	29	71	2	3	NW	2 fair
12	9 a m	29	71	2	3	NW	1 fair
	4 p m	29	71	2	3	SW	2 cloudy
13	9 a m	29	71	2	3	SW	2 cloudy
	4 p m	29	71	2	3	SW	3 cloudy
14	9 a m	29	71	2	3	SW	2 cloudy
	5 p m	29	71	2	3	SW	1 cloudy
15	9 a m	29	71	2	3	SW	4 cloudy
	5 p m	29	71	2	3	SW	2 fair
16	9 a m	29	71	2	3	SW	2 fair
	5 p m	29	71	2	3	SW	

0,194

AND OBSERVATIONS. 13

NOVEMBER 1733.

B. Hour.	Baro. In D.	Ther. In D.	Hyg I. D.	Wind. Dir. For.	Weather.	Rain.
17 9 a m	19 5	10 6	2 0	S	4 cloudy	0,194
5 p m	19 6	9 9	1 9	W	2 cloudy	
18 9 a m	19 8	9 7	2 0	S W	2 fair	
4 p m	19 8	10 4	1 9	S W	2 fair	
19 9 a m	19 7	9 6	2 0	S W	2 fair	
4 p m	19 8	10 4	2 0	S W	2 fair	
20 9 a m	19 7	11 0	2 1	S	2 cloudy	
4 p m	19 6	10 6	1 9	S	2 cloudy	
21 9 a m	19 5	10 9	2 0	S E	2 fair	
4 p m	19 6	11 3	2 0	S E	2 fair	
22 9 a m	19 8	11 7	1 1	S	1 fair	
4 p m	19 8	11 9	2 0	S	1 fair	
23 9 a m	19 8	11 5	2 1	S W	2 fair	
4 p m	19 7	11 5	2 0	S W	3 cloudy	
24 9 a m	19 7	11 9	2 7	S W	2 cloudy	
4 p m	19 6	12 4	2 4	S W	2 cloudy	
25 9 a m	19 7	10 2	1 9	W	3 fair	0,037
4 p m	26 7	10 3	1 9	W	3 fair	
26 9 a m	19 7	10 2	2 0	W	3 fair	
4 p m	29 5	9 4	2 5	W	4 rain	
27 9 a m	19 8	9 5	1 8	W	2 fair	
4 p m	29 9	10 2	1 8	W	2 cloudy	
28 9 a m	19 8	11 0	2 4	W	2 fair	
4 p m	29 7	11 1	2 9	W	3 cloudy	
29 9 a m	19 8	11 0	2 0	W	2 cloudy	0,095
4 p m	29 7	11 8	2 2	W	3 rain	
30 9 a m	19 5	11 5	2 0	S W	3 fair	
4 p m	29 6	9 7	1 9	S W	3 cloudy	

H at a med. 29

7 10 8 2 0

Total depth 0,326

Gr. height 30

3 12 7 2 7

L. height 29

1 8 2 1 7

DECEMBER 1733.

D.	Hour.	Baro. In. D.	Ther. In. D.	Hyg. I. D.	Wind. Dir. For.	Weather.	Rain. In D.
29	a m	29	6 9	3 2	0 W	2 fair	0,205
	4 p m	29	6 9	2 2	0 W	2 fair	
29	a m	29	4 12	0 2	6 W	4 cloudy	0,146
	4 p m	29	3 11	1 2	1 W	4 fair	
30	a m	29	7 9	6 1	7 W	2 fair	0,072
	4 p m	29	9 10	1 1	9 W	2 fair	
30	a m	30	0 11	2 2	2 W	2 cloudy	
	4 p m	30	1 11	4 2	3 W	2 fair	
30	a m	30	2 11	1 2	1 W	1 cloudy	
	4 p m	30	2 11	4 2	1 S W	2 cloudy	
30	a m	30	2 10	7 2	0 S W	2 cloudy	
	4 p m	30	1 11	1 1	9 S W	1 cloudy	
30	a m	30	0 10	0 2	0 S W	1 cloudy	
	4 p m	30	0 10	2 1	9 S W	1 cloudy	
30	a m	30	0 9	7 2	0 S W	1 fair	
	4 p m	29	9 10	7 2	0 S W	1 fair	
30	a m	29	7 9	0 1	9 S W	1 fog	
	4 p m	29	6 9	5 1	9 S W	1 cloudy	
30	a m	29	1 10	5 2	2 S	2 cloudy	
	4 p m	29	3 10	2 2	1 S W	2 cloudy	
31	a m	29	c 11	5 2	4 S W	4 rain	0,248
	4 p m	29	0 12	c 2	2 S W	4 rain	
31	a m	29	1 10	7 1	9 S	1 fair	0,153
	4 p m	29	0 11	8 2	1 S	1 fair	
31	a m	29	1 10	7 2	2 S	0 fog	0,124
	4 p m	29	c 10	8 2	1 S W	2 cloudy	
31	a m	29	c 10	9 2	0 S	2 fog	0,176
	4 p m	28	8 11	4 2	2 S	2 cloudy	
31	a m	29	2 11	1 2	1 S W	3 cloudy	0,198
	4 p m	29	3 10	9 1	9 S W	4 cloudy	
31	a m	29	2 10	3 2	2 S	2 rain	0,354
	4 p m	29	3 9	9 2	3 N W	1 fair	

1,666

D.

179

4

189

4

199

4

209

4

219

4

229

4

239

4

249

4

259

4

269

4

279

4

289

4

299

4

309

4

319

4

Hata

Gr. h

L. h

AND OBSERVATIONS. 15

DECEMBER 1733.

Rain. In D.	D.	Hour	Baro. In D.	Ther. In D.	Hyg l. D.	Wind. Dir. For.	Weather.	Rain.
	17	9 a m	29	4 10	0 2	4 W	2 ra'n	1,666
0,205		4 p m	29	4 10	3 2	5 W	1 fair	0,565
	18	9 a m	29	4 12	6 2	3 S W	4 fair	0,213
0,146		4 p m	29	4 12	5 2	5 S W	3 fair	
	19	9 a m	29	6 11	5 2	2 S W	2 fair	
0,072		4 p m	29	7 11	7 2	2 W	1 fair	
	20	9 a m	29	7 11	7 2	2 S E	1 cloudy	0,274
		4 p m	29	7 11	7 2	2 S W	1 cloudy	
	21	9 a m	29	6 11	9 2	3 S	0 fog	0,074
		4 p m	29	6 11	3 2	1 W	1 fair	
	22	9 a m	29	5 10	3 2	1 S W	1 fair	0,155
		4 p m	29	1 10	7 2	2 S W	3 cloudy	
	23	9 a m	29	2 10	2 1	9 S W	2 fair	0,130
		4 p m	29	3 10	6 1	8 S W	2 fair	
	24	9 a m	29	3 9	7 2	8 S W	2 fair	0,195
		4 p m	29	5 9	7 2	0 W	2 fair	
	25	9 a m	29	4 9	9 2	1 W	2 cloudy	0,055
		4 p m	29	4 9	5 2	1 W by S	2 cloudy	
	26	9 a m	29	0 8	9 2	1 W by S	2 fair	0,124
		4 p m	29	7 9	4 2	0 W by S	2 fair	
0,248	27	9 a m	29	2 9	0 2	2 W	1 fog	
		4 p m	29	4 10	4 2	2 W	1 fair	
0,153	28	9 a m	29	8 10	2 2	3 S W	0 fog	0,093
		4 p m	29	9 10	6 2	1 S W	1 cloudy	
0,124	29	9 a m	29	6 11	7 2	1 S W	3 cloudy	
		4 p m	29	6 10	7 1	9 S W	2 fair	
0,176	30	9 a m	29	5 11	0 2	0 S W	3 rain	0,075
		4 p m	29	4 11	7 2	2 S W	3 fair	
0,198	31	9 a m	29	2 11	3 2	2 S W	3 cloudy	
		4 p m	29	3 11	1 2	1 W	2 cloudy	
0,354	H. at a med.		29	5	10	7 2	1	Total depth 3,629
	Gr. height		30	2	12	6 2	8	
1,666	L. height		28	8	8	9 1	7	

JANUARY 1734.

D.	Hour.	Baro.	Ther.	Hyg.	Wind.	Weather.	Rain.
		In. D.	In. D.	I. D.	Dir. For.		In D.
1	9 a m	29	4	8	6 2 3	S W	3
	4 p m	29	6	9	3 2 0	W	3
2	9 a m	29	6	9	6 1 0	S	3
	4 p m	29	4	9	3 2 1	S W	2
3	9 a m	29	3	8	4 2 3	W	2
	4 p m	29	3	8	5 2 1	W	2
4	9 a m	29	2	8	3 2 2	S W	2
	4 p m	29	2	8	3 2 1	S W	2
5	9 a m	29	2	7	8 2 3	S W	2
	4 p m	29	3	7	3 2 5	S W	2
6	9 a m	29	6	7	4 2 0	W	2
	4 p m	29	6	7	7 1 9	W	1
7	9 a m	29	7	7	1 1 9	W	2
	4 p m	29	8	7	7 2 6	W	1
8	9 a m	30	0	8	0 2 8	S W	1
	4 p m	30	0	8	8 2 6	S W	2
9	9 a m	30	0	9	0 2 0	S by W	1
	4 p m	30	0	9	3 2 0	S	1
10	9 a m	30	2	8	7 2 5	S	1
	4 p m	30	3	9	1 2 5	S	1
11	9 a m	30	5	7	7 1 8	S	1
	4 p m	30	5	7	5 1 7	S	1
12	9 a m	30	5	7	0 1 7	S W	1
	4 p m	30	6	7	6 1 6	S W	1
13	9 a m	30	5	6	2 1 5	S W	1
	4 p m	30	4	7	4 1 7	S W	2
14	9 a m	30	2	7	0 2 0	S W	1
	4 p m	30	1	7	4 2 2	S W	1
15	9 a m	30	0	7	0 2 6	S by E	1
	4 p m	29	9	7	3 2 5	S by E	1
16	9 a m	29	7	7	6 2 5	S	1
	4 p m	29	7	8	2 2 3	S	1

0,408

D.	H.
17	9
4	
18	9
4	
19	9
4	
20	9
4	
21	9
4	
22	9
4	
23	9
4	
24	9
4	
25	9
4	
26	9
4	
27	9
4	
28	9
4	
29	9
4	
30	
31	
H.	
Gr	
L.	

AND OBSERVATIONS. 17

JANUARY 1734.

Rain. In D.	D	Hour.	Baro. In. D.	Ther. In. D.	Hyg. l. D.	Wind. Dir. For.	Weather.	Rain.
	17	9 a m	29 8 8	0 2	5 S		1 fog	0,408
		4 p m	30 0 8	5 2	4 S W		1 cloudy	
	18	9 a m	29 9 10	0 2	1 S W		3 cloudy	
		4 p m	30 0 11	2 2	3 S W		1 cloudy	
	19	9 a m	30 0 10	7 2	3 S W		1 cloudy	
		4 p m	30 1 10	5 2	1 S W		2 fair	
	20	9 a m	30 1 9	8 2	0 S W		1 fair	
		4 p m	30 1 9	8 2	0 S W		1 fair	
	21	9 a m	30 0 8	9 2	0 S W		1 fair	
0,085		4 p m	29 8 10	0 1	9 S W		1 fair	
	22	9 a m	29 4 9	4 2	1 S W		2 fair	
		4 p m	29 4 9	7 2	1 W		2 fair	
	23	9 a m	29 8 9	5 1	9 W		2 fair	
		4 p m	29 8 10	4 1	2 W		2 fair	
	24	9 a m	29 8 11	1 2	5 W		2 cloudy	
0,055		4 p m	29 7 11	5 2	4 W		2 rain	
	25	9 a m	29 6 10	4 2	1 W by S		3 cloudy	0,065
		4 p m	29 3 9	9 2	1 W by S		3 cloudy	
	26	9 a m	29 8 10	1 2	0 N W		2 fair	
		4 p m	29 9 10	8 1	8 W		3 fair	
0,095	27	9 a m	30 2 11	0 2	6 W		2 fair	
		4 p m	30 2 11	2 2	3 W		3 cloudy	
	28	9 a m	30 2 10	7 2	5 S W		2 fair	
		4 p m	30 2 11	4 2	4 W		2 fair	0,045
	29	9 a m	30 3 11	0 2	5 W		2 cloudy	
		4 p m	30 3 10	8 2	5 W by S		2 cloudy	
	30	9 a m	30 3 11	2 2	5 W		2 cloudy	0,075
		4 p m	30 2 11	2 2	3 W		2 cloudy	
0,173	31	9 a m	30 2 10	2 2	1 W		2 fair	
		4 p m	30 2 9	4 1	5 N W		3 fair	
Total depth 0593								
0,408	H. atamed. 29 9 8 8 2 2							
	Gr. height 30 6 11 5 2 8							
	L. height 29 2 6 2 1 2							

18 MEDICAL ESSAYS

FEBRUARY 1734.

D.	Hour.	Baro. In. D.	Ther. In. D.	Hyg. I. D.	Wind. Dir. For.	Weather.	Rain. In D.
29	a m 30	29	01	9	W by S	2 fair	
	4 p m 30	110	42	1	W	3 cloudy	
29	a m 30	010	62	1	W	4 fair	
	4 p m 30	011	02	0	W	3 fair	
30	a m 19	910	82	3	S W	2 fair	
	4 p m 19	711	22	2	W	3 fair	
40	a m 19	99	91	9	W	3 fair	0,114
	5 p m 19	810	32	0	W	3 cloudy	
50	a m 19	810	82	3	W	2 cloudy	
	4 p m 19	911	42	1	W	2 cloudy	
60	a m 19	810	92	2	S W	2 cloudy	
	4 p m 19	611	02	0	S W	3 cloudy	
70	a m 19	610	32	0	W by N	3 fair	0,094
	4 p m 19	810	41	4	N W	3 fair	
80	a m 30	39	91	6	W	1 cloudy	
	4 p m 30	310	52	0	W	2 cloudy	
90	a m 30	211	02	3	S by W	3 cloudy	
	4 p m 30	011	32	0	S W	2 cloudy	
100	a m 19	910	42	0	N W	3 fair	0,243
	4 p m 30	010	61	8	N W	3 fair	
110	a m 20	110	71	9	S W	2 cloudy	
	4 p m 19	911	11	8	S W	2 cloudy	
120	a m 19	611	72	0	S W	2 cloudy	
	4 p m 19	611	81	7	S W	1 cloudy	
130	a m 19	411	82	0	S W	2 fair	0,013
	4 p m 19	511	51	6	S W	2 fair	
140	a m 19	69	81	7	S W	2 fair	
	4 p m 19	59	61	5	S W	3 cloudy	
150	a m 18	810	52	0	S by W	4 rain	
	4 p m 18	610	22	0	S W	4 cloudy	
160	a m 20	09	22	0	S W	3 fair	
	4 p m 19	09	42	0	W	2 cloudy	

0,474

AND OBSERVATIONS.

19

FEBRUARY 1734.

Rain. In D.	D. Hour.	Baro. In. D.	Ther In. D.	Hyg. In. D.	Wind. Dir. For.	Weather.	Rain.
							0,474
	17 9 a m	29 0 9	21 8	8	S W	3 cloudy	
	5 p m	29 1 8	6 2	2	S W	3 cloudy	
	18 9 a m	29 0 9	9 2	1	N W	3 cloudy	
	5 p m	29 5 10	4 1	5	N	4 fair	
0,114	19 9 a m	29 7 9	11 7	N W	2 fair	0,065	
	4 p m	29 5 10	2 1	7	W	2 rain	
	20 9 a m	29 5 9	12 0	N W	3 cloudy		
	5 p m	29 7 9	7 1	5	N W	2 fair	
	21 9 a m	29 9 9	11 9	W	3 cloudy		
	4 p m	29 9 9	6 2	0	W	2 rain	
0,094	22 9 a m	29 7 10	9 2	6	S W	2 fair	
	5 p m	29 5 11	6 2	1	S W	2 cloudy	
	23 9 a m	29 3 0	9 2	1	S W	2 cloudy	
	5 p m	29 3 11	5 1	7	S W	2 cloudy	
	24 9 a m	28 9 10	6 2	0	S W	2 cloudy	
	5 p m	28 9 10	6 1	7	W	1 fair	
0,243	25 9 a m	29 0 10	1 2	2	S	1 cloudy	
	5 p m	29 1 10	8 2	1	S	1 cloudy	
	26 9 a m	29 3 9	6 2	1	S	1 fair	0,056
	4 p m	29 3 10	8 2	0	S	1 fair	
	27 9 a m	29 6 9	7 2	0	S W	0 fair	
	4 p m	29 6 11	4 1	6	S W	2 cloudy	
0,013	28 9 a m	29 4 11	4 1	8	S	4 cloudy	
	5 p m	29 5 12	2 1	5	S W	3 fair	
	H. at a med. 29 6 10 5 1 9						Total depth 0,595
	Gr. height 30 3 12 2 2 6						
	L. height 28 6 9 6 1 4						

0,474

MARCH 1734.

D.	Hour	Baro. In D	Ther. In D.	Hyg. l. D.	Wind. Dir. For.	Weather.	Rain. In D.				
1	0 a m	29	7	10	7	1	8	S	1	fair	
	5 p m	29	7	11	7	1	6	S E	2	fair	
2	0 a m	29	6	10	7	1	7	S by E	1	cloudy	
	5 p m	29	6	11	4	2	0	S E	1	fair	
3	0 a m	29	6	11	5	2	1	S W	1	cloudy	
	5 p m	29	6	11	9	1	8	S W	1	fair	
4	0 a m	29	5	12	1	1	9	S	2	cloudy	
	6 p m	29	4	11	5	1	8	S	1	cloudy	
5	0 a m	29	5	11	2	1	8	S W	1	rain	
	5 p m	29	4	11	6	1	6	S W	1	cloudy	
6	0 a m	29	1	12	3	2	0	S W	3	cloudy	0,195
	4 p m	29	2	12	3	1	7	S W	3	fair	
7	0 a m	29	2	11	6	1	7	W	3	fair	
	5 p m	29	4	11	3	1	7	W by N	3	fair	
8	0 a m	29	7	9	6	1	8	W by N	2	fair	0,055
	5 p m	29	7	10	5	1	6	W by N	2	cloudy	
9	0 a m	29	6	11	6	1	6	W	2	cloudy	0,025
	5 p m	29	6	10	8	1	6	W	2	cloudy	
10	0 a m	29	7	9	9	2	0	W by N	2	fair	0,048
	5 p m	29	8	10	7	1	5	N W	2	fair	
11	0 a m	29	8	9	9	1	8	S E	2	fair	0,036
	6 p m	29	6	11	1	1	9	S	2	cloudy	
12	0 a m	29	6	10	8	1	8	S W	3	rain	0,095
	5 p m	29	7	10	7	1	6	W	3	fair	
13	0 a m	29	5	9	8	1	7	S E	2	fog	
	5 p m	29	3	10	6	1	9	S E	1	rain	
14	0 a m	29	6	9	5	2	0	S by E	0	fair	0,172
	5 p m	29	5	11	5	1	7	S E	0	cloudy	
15	0 a m	29	4	10	4	1	0	W	3	cloudy	0,055
	5 p m	29	6	11	3	1	4	W by N	2	fair	
16	0 a m	29	7	11	3	1	8	S by W	2	cloudy	0,026
	6 p m	29	5	11	5	1	6	S W	2	cloudy	

0,707

D.	H.
17	9
5	
18	9
16	
19	9
5	
20	9
6	
21	9
5	
22	9
5	
23	9
6	
24	9
5	
25	9
5	
26	9
4	
27	9
6	
28	9
6	
29	9
5	
30	9
5	
31	9
5	
H at a	
Gr. h	
L. h	

AND OBSERVATIONS. 21

MARCH 1734.

Rain. In D.	D. Hour.	Baro. In D.	Ther. In D.	Hyg l. D.	Wind. Dir. For.	Weather.	Rain.
	17 9 a m	29 8	9 6	1 8	S W	3 fair	0.707
	5 p m	29 8	10 8	1 3	W by S	4 cloudy	0.034
	18 9 a m	29 5	10 6	1 9	S by W	1 cloudy	0.195
	6 p m	29 4	11 3	1 9	S by W	1 cloudy	
	19 9 a m	29 4	10 8	1 9	S by E	0 fair	0.075
	5 p m	29 4	11 0	1 4	S W	1 fair	
	20 9 a m	29 2	11 2	1 8	S E	1 rain	0.113
	6 p m	29 1	12 5	1 9	S by W	1 cloudy	
	21 9 a m	29 1	11 5	1 8	S W	1 fair	0.093
	5 p m	29 4	11 0	1 7	W	2 fair	
0.195	22 9 a m	29 5	11 2	0	S W	2 fair	0.204
	5 p m	29 5	12 5	1 4	S W	3 cloudy	
	23 9 a m	29 4	11 8	1 7	S W	4 variable	0.265
	6 p m	29 4	10 7	1 5	S W	4 rain	
0.055	24 9 a m	29 6	10 8	1 7	S W	3 fair	
	5 p m	29 6	11 4	1 4	S W	2 cloudy	
0.015	25 9 a m	29 6	10 6	1 7	S W	2 fair	
	5 p m	29 6	11 5	1 3	W by S	2 fair	
0.048	26 9 a m	29 3	11 3	1 7	S W	1 cloudy	0.210
	4 p m	29 1	11 4	1 8	S W	1 rain	
0.036	27 9 a m	29 4	11 3	1 6	N W	2 fair	0.034
	6 p m	29 5	11 4	1 4	N W	2 fair	
0.095	28 9 a m	29 7	9 6	1 6	N W	1 fair	
	6 p m	29 6	11 0	1 5	W	1 cloudy	
	29 9 a m	29 5	11 2	1 5	W by N	3 fair	0.044
	5 p m	29 5	11 4	1 4	W by N	3 fair	
0.175	30 9 a m	29 7	11 2	1 5	W by N	2 fair	0.085
	5 p m	29 7	12 3	1 7	S W	3 cloudy	
0.055	31 9 a m	29 8	12 4	1 1	W by N	3 cloudy	0.063
	5 p m	29 9	12 4	1 7	W by N	2 fair	
0.026	H at a med. 29		5 11	11 7	Total depth 2.123		
0.707	Gr. height 29		9 12	5 1			
	L. height 29		1 9	5 1			

APRIL 1734.

D.	Hour.	Baro. In. D.	Ther. In. D.	Hyg. I. D.	Wind. Dir. For.	Weather.	Rain. In D.
1	9 a m	29	9 12	3 2	0 S W	2 cloudy	
	6 p m	29	9 12	8 1	7 S W	3 cloudy	
2	9 a m	29	9 12	2 1	9 S W	3 fair	0,125
	4 p m	30	0 13	2 1	8 W	2 cloudy	
3	9 a m	30	0 12	4 1	9 W	2 cloudy	
	5 p m	30	0 12	6 1	7 W	2 cloudy	
4	9 a m	29	9 12	3 2	0 W	2 fair	
	4 p m	29	9 13	7 1	5 W	2 fair	
5	9 a m	30	0 11	3 1	7 W	2 fair	0,034
	5 p m	30	0 12	4 1	3 N E	2 fair	
6	9 a m	29	8 13	3 1	5 S E	0 cloudy	
	5 p m	29	6 13	1 1	6 S W	2 cloudy	
7	9 a m	29	6 10	9 1	5 W	2 fair	
	6 p m	29	6 11	1 1	7 W	3 fair	
8	9 a m	29	6 11	5 1	8 S W	2 cloudy	
	4 p m	29	5 11	7 1	5 W by N	2 fair	
9	9 a m	29	5 10	5 1	6 W by N	3 fair	
	4 p m	29	6 11	9 1	2 N W	3 fair	
10	9 a m	29	8 12	1 2	0 W by S	3 cloudy	
	5 p m	29	8 12	4 1	5 W	3 cloudy	
11	9 a m	29	9 11	4 1	6 W	2 fair	
	6 p m	30	0 11	6 1	4 W	4 variable	
12	9 a m	30	0 11	8 1	5 W by S	2 cloudy	
	5 p m	30	0 12	7 1	5 S W	3 cloudy	0,055
13	9 a m	30	1 13	2 1	9 W	2 fair	
	5 p m	30	2 13	7 1	5 W by S	2 fair	
14	9 a m	30	2 12	7 1	9 W by S	1 fair	
	5 p m	30	1 14	7 1	4 S by E	1 fair	
15	9 a m	30	0 12	5 1	8 S E	1 fair	
	5 p m	29	9 14	0 1	3 S E	1 fair	
16	9 a m	29	9 12	3 1	8 S E	1 fair	
	5 p m	29	9 13	4 1	3 E	0 fair	

0,114

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AND OBSERVATIONS. 23

APRIL 1734.

D.	Hour	Baro. In D.	Ther. In. D.	Hyg. I. D.	Wind. Dir. For.	Weather.	Rain.
17	9 a m	29 9	11 7	1 9	E by N	2 fair	0,214
	5 p m	29 9	11 8	1 6	N E	2 fair	
18	9 a m	29 7	10 7	2 4	E	3 cloudy	0,137
	4 p m	29 8	11 5	1 8	E	2 cloudy	
19	9 a m	30 0	11 5	1 9	E	2 fair	0,204
	6 p m	30 0	11 2	1 8	E	2 cloudy	
20	9 a m	30 0	11 5	2 6	E	2 cloudy	0,055
	7 p m	30 0	11 1	2 0	E	2 fair	
21	9 a m	30 0	11 5	1 6	E	1 fair	
	6 p m	29 9	12 6	1 4	E	1 fair	
22	9 a m	29 9	9 9	1 8	N E	2 cloudy	0,045
	6 p m	29 9	9 4	2 1	E	1 rain	
23	9 a m	29 8	11 3	2 0	E	1 cloudy	
	6 p m	29 8	11 4	1 8	E	2 fair	
24	9 a m	29 7	11 4	2 0	S E	2 cloudy	
	6 p m	29 6	11 6	1 1	S E	2 cloudy	
25	9 a m	29 5	12 7	2 3	S E	1 fair	
	6 p m	29 6	13 0	1 6	W	1 cloudy	
26	9 a m	29 5	13 5	1 6	S W	2 fair	0,056
	6 p m	29 5	13 1	1 4	W	2 fair	
27	9 a m	29 5	12 6	1 6	S W	2 fair	0,084
	5 p m	29 5	13 0	1 6	W	1 cloudy	
28	9 a m	29 5	12 4	1 6	W	2 fair	0,095
	6 p m	29 5	13 3	1 3	S W	2 fair	
29	9 a m	29 5	13 5	1 3	S W	2 fair	0,116
	6 p m	29 4	13 6	1 4	S W	1 rain	
30	9 a m	29 4	12 6	1 4	S W	2 fair	
	5 p m	29 4	12 4	1 4	S W	2 cloudy	
H at a med. 29		8 12	2 1	7	Total depth 1,006		
Gr. height 30		2 14	7 2	6			
L. height 29		4 9	4 1	2			

MAY 1734.

D.	Hour.	Baro. In D.	Ther. In D.	Hyg. In D.	Wind. Dir. For.	Weather.	Rain. In D.
19	a m 29	4 12	2 1	3	S W	2 cloudy	
5	p m 29	4 12	0 1	5	S W	2 rain	
27	a m 29	7 9	8 1	7	N E	2 fair	
5	p m 29	7 10	8 1	4	N E	2 fair	0,066
39	a m 29	8 10	4 1	7	N	2 cloudy	
6	p m 29	8 11	6 1	2	N by W	2 fair	
49	a m 29	8 11	0 1	4	N W	2 fair	
5	p m 29	9 12	3 1	0	N W	2 fair	
59	a m 30	0 11	2 1	2	W	1 fair	0,057
5	p m 29	9 12	6 1	6	W	2 cloudy	
69	a m 29	7 11	9 1	5	W	2 cloudy	
7	p m 29	8 11	2 1	5	W	1 cloudy	
79	a m 29	7 11	4 1	5	W	1 cloudy	
5	p m 29	6 10	8 1	7	N E	1 cloudy	
89	a m 29	7 12	0 1	6	E	2 fair	0,117
5	p m 29	8 13	1 1	3	E	2 fair	
59	a m 30	0 11	8 1	4	N E	2 fair	0,055
5	p m 30	1 12	7 1	6	N E	2 fair	
10	a m 30	0 12	0 1	5	W by N	2 cloudy	
5	p m 29	9 12	8 1	3	N W	3 fair	
119	a m 29	2 12	2 1	5	W	4 cloudy	
7	p m 29	4 12	4 1	5	W	3 cloudy	
129	a m 29	4 11	5 1	4	N W	3 fair	
5	p m 29	3 12	6 1	0	N W	3 cloudy	
139	a m 29	4 11	0 1	3	N W	3 fair	
5	p m 29	4 11	9 1	2	S	1 hail	
149	a m 29	4 11	0 1	4	E	2 fair	0,185
5	p m 29	4 11	3 1	3	E	2 fair	
159	a m 29	5 10	6 1	4	N W	1 fair	0,205
7	p m 29	5 11	7 1	3	N	1 cloudy	
169	a m 29	6 11	9 1	5	E by S	1 cloudy	0,113
7	p m 29	6 11	9 1	5	E	1 rain	
							0,808

D. H.

17 9

6

18 9

6

19 9

6

10 9

6

21 9

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22 9

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23 9

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24 9

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25 9

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26 9

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27 9

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28 9

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29 9

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30 9

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31 9

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V

AND OBSERVATIONS. 25

MAY 1735.

D. Hour.	Baro.	Ther.	Hyg	Wind.	Weather.	Rain.
(In D.)	(In D.)	(In D.)	(In D.)	Dir. For.		
17 ⁹ a m	29 6	11 2	3	NW	1 fair	0.808
6 p m	29 6	14 6	2	E	2 fair	0.830
18 ⁹ a m	29 5	12 6	4	NE	1 fair	
6 p m	29 5	12 2	4	SW	2 fair	
19 ⁹ a m	29 5	11 8	3	SW	1 fair	0.145
6 p m	29 6	11 9	2	SW	1 fair	
20 ⁹ a m	29 8	12 3	7	W	2 cloudy	0.070
6 p m	29 8	13 6	4	W	2 cloudy	
21 ⁹ a m	29 8	13 9	4	S	1 fair	0.110
6 p m	29 6	12 6	7	SW	2 cloudy	
22 ⁹ a m	29 4	13 1	7	SW	1 cloudy	0.398
7 p m	29 4	13 1	4	SW	1 cloudy	
23 ⁹ a m	29 6	12 2	5	W	2 fair	0.127
7 p m	29 7	12 5	3	W	2 fair	
24 ⁹ a m	29 8	13 2	5	W	1 cloudy	0.088
6 p m	29 8	13 7	3	E	1 fair	
25 ⁹ a m	29 8	12 8	5	E	2 fair	0.036
8 p m	29 7	11 5	6	E	2 cloudy	
26 ⁹ a m	29 6	11 7	0	NE	2 cloudy	
7 p m	29 7	11 7	1	NE	2 cloudy	
27 ⁹ a m	29 7	12 6	8	NE	1 fair	
6 p m	29 7	13 6	6	E	1 fair	
28 ⁹ a m	29 7	13 6	0	NE	1 fair	0.155
7 p m	29 7	13 3	3	SE	1 cloudy	
29 ⁹ a m	29 6	12 7	2	E	1 cloudy	0.085
6 p m	29 6	12 2	2	E	1 cloudy	
30 ⁹ a m	29 5	12 2	5	E by N	2 cloudy	0.840
5 p m	29 5	13 6	6	E by N	2 cloudy	
31 ⁹ a m	29 5	12 3	4	NE	3 rain	0.213
5 p m	29 6	11 3	3	NE	3 rain	
Total depth 3.373						
H. at med. 29	8	12	11	5		
Gr. height 30	1	13	9	5		
L. height 29	3	9	8	0		

VOL. III.

C

II. An

II. *An Account of the DISEASES that were most frequent last Year in Edinburgh.*

IN June 1733, several inhabitants of Edinburgh were seized with tertian agues, and others laboured under slight fevers, with pain of the head, and flying pains through the body, the sick having short remissions of the fever, and partial sweatings. At the same time scarlet fevers and sore throats were frequent in several parts of the country near the city.

In July many complained of anginae, coughs, hoarseness, and other effects of what they call summer-colds, and children were attacked with the scarlet-fever and angina, which became very epidemic in the two succeeding months, was less frequent and milder in October; but continued all the winter and spring. This disease began commonly with a quick pulse, heat, thirst, head-ach, and a pain in the throat, where frequently a swelling of the amygdalae was observed. Many had a vomiting and diarrhoea, at the first attack of this disease, without any remarkable change on the other symptoms. After a day or two, the face, or extremities, and sometimes the whole body swelled, the skin being red, with a watery clearness shining through it. Frequently the swelling and redness proceeded gradually from one part to another. It was remarked, that such patients who had undergone the scarlet-fever any time of their lives before, took at this time the fever and angina, without the scarlet eruption,

eruption, but all who laboured under the scarlet-fever had the angina also.

Many who were neglected in the beginning of this disease, were suffocated by the angina. Few died who were timely and plentifully bled, which weakened the fever, relieved the throat, and was the only medicine that removed the vomiting and diarrhoea. After the pulse was brought down with the loss of blood, vesicatories were of use, and the cure was afterwards hastened and completed by purging the patients with aperient laxative ptizans.

In the account of epidemical diseases in vol. 2. we mentioned the small-pox having attacked some children through all the spring of 1733, the numbers increasing in May, which they continued to do in June, July, and August, but were then generally of the distinct mild kind. As they became more frequent in September, there were more of them confluent, and the number of patients increased all October, after which they raged most violently, few families escaping them till February, when they decreased, and gradually went off in March. The pocky pimples generally began to appear the third day from the sickning, and yet were not always either confluent or dangerous. Very few had any purple spots with the small-pox; and, of those who recovered of the confluent kind, fewer had tumors or ulcers than at other times.

The cool regimen was generally followed; and, when the fever was high in the beginning, with the head or breathing much affected, the

patients were commonly blooded and vomited; and, in the confluent kind, it was necessary and proved useful to repeat the bleeding about the height of the disease, and some time after. From the first attack of the fever, till the small-pox were all out, many ordered pediluvia of warm water once or twice a-day, which seemed in many cases to relieve the head considerably, and to bring a greater number of pimples to the lower extremities. When the patients were costive, diluent cooling clysters were injected. Syrup of white poppies was generally given at night to procure sleep. In some who had bloody stools and urine, the spirit of vitriol, mixed in pretty large quantities with the drink of the sick, did very remarkable service, these threatening symptoms disappearing in a little time after they began to take such acidulated liquors. Vesicatories assisted the eruption when the pulse was low, and often were of service in removing raving, startings of the nerves, convulsions, and difficult laborious breathing; and some who had a bad confluent small-pox, seemed to have the dangerous symptoms at the blackening prevented, by applying vesicatories a day before the blackening began, and by keeping up a suppuration in the blistered parts for some days. Gentle emetics were also advantageously given, when the stomach or lungs appeared to be overcharged with mucus. When the small-pox were empty, or had only a little watery matter in them, and the swellings began to fall suddenly on the 9th or 10th day, purgatives were given to some with good success.

Though

Though bleeding in the beginning of the small-pox evidently gave relief in a great many cases, yet it could not well be judged, whether the taking blood before the varjalous fever began, or after the symptoms appeared, had any effect in determining the nature or number of the small-pox; for many, who had been prepared by bleeding, purging, issues, and low cooling diet, had a very bad confluent small-pox; and others who had also been treated in that manner, and a great number, who had used no such precautions, took the mild kind. Some who had undergone courses of mercury, and who had been afterwards kept for a considerable time to the constant use of *Æthiops mineral*, were seized with the confluent small-pox and died.

During the harvest months of 1733, dysenteries were frequent and mortal in Fife, especially on the coast of the Frith of Forth.

In March and April 1734, tertian agues became frequent in Edinburgh, and were very irregular; but repeated vomits either carried them off, or made them easily yield to the bark. At the same time many children in the neighbouring villages were seized with a very sharp fever, and high laborious breathing, which soon killed them, unless they were timely relieved by frequent and plentiful bleeding, and gentle vomits.

30 MEDICAL ESSAYS

III. *An Extract from the public Register of Burials in Edinburgh.*

1733.	Men	Women.	Child.	Still-born.	Sum.
June -	19	29	40	4	92
July - -	17	19	41	2	79
August -	20	26	63	4	113
September	13	21	65	3	102
October	15	26	106	6	153
November	19	27	144	8	198
December	21	27	116	4	168
1734.					
January	26	46	80	3	155
February	22	23	57	4	106
March -	28	27	66	4	125
April -	25	31	50	3	109
May - -	31	46	41	9	127
Total	256	348	869	54	1527

IV. *The Effects of the Conessi Bark, by——.*

Gentlemen,

“NOT having allowance from my friend, who wrote me the inclosed letter, to publish his name, I cannot take upon me to communicate it; but can assure you, his integrity, good sense, and knowledge, are such as may give entire credit to his information; and lately I cured an obstinate dysentery of three months standing, which had yielded nothing to a great variety of other medicines, by giving the Conessi bark in the form prescribed by him. I am

Your most humble servant,

ALEX. MONRO.”

T H E

THE tree, of which I gave you some of the bark as a specific in diarrhoeas, grows on the Cormandel coast in the East-Indies, where it is called Conessi, and is not unlike the Cado-gapala of the *Hortus Malabaricus*. The conessifeca, or conessi-bark of the small young branches of the tree which has least moss, or external insipid scurf on it, is to be chosen, and all that scurf is to be scraped off.

The clean bark being pounded into a very fine powder, is made into an electuary, with syrup of oranges, and taken to the quantity of half a drachm, or more, four times a-day, for three or four days. The first day it increases the number and quantity of the stools, but without increasing the gripes. The second day the bark is taken, the colour of the stools is mended; and on the third or fourth day, their consistence generally comes near to a natural state, when it succeeds at all.

In recent diarrhoeæ, arising from irregularities in diet, without a fever, this medicine seldom fails to make a cure, if a vomit of ipecacuanna is given immediately before the patient begins the use of the bark. The same management also is attended commonly with success in persons of a lax habit of body, who are troubled with an habitual diarrhoea in moist rainy weather, a remarkable itching in the skin being felt on the third or fourth day. To such patients especially, the electuary ought to be given morning and evening, for some time after they are seemingly cured. Their drink should be water wherein rice hath been boiled,

and

and sometimes emulsions of the cold seeds, with *sal prunell.* are necessary.

If there is a fever with the looseness, that must be removed by bleedings and cool emulsions, or *decoct. alb.* with *sal prunell.* before the *conessi* bark is given.

Sometimes when the cause of a diarrhoea, stopped by this medicine, lies beyond the intestinal canal, the patient, in a few days after, complains of a pain in the right hypochonder, or in the right shoulder, or over the stomach towards the left side, causing often a dull sense of pain, near or above the left clavicle, with a feverish pulse. As soon as these symptoms appear, the patient must be bled, and his blood will be fizy, or with a tough yellowish crust on the top, when it has coagulated. The quantity of blood to be taken away, and the repeating the venæsection, must be determined by the patient's strength, the degree of fever, and sharpness of his pain. In such cases however the bleeding seldom removes the pain entirely; but, after the fever is brought sufficiently down by the loss of blood, I have seldom missed to complete the cure, by giving sweet mercury, or rather calomel for some days, in small quantities, as an alterative.

I ought to observe, that the bark should be fresh-powdered, and the electuary new made every day or second day, otherwise the bark loses its austere but grateful bitterness on the palate, and its proper effects on the intestines.

V. *A Gangrene stopped by the Cortex Peruvianus; by Mr SAMUEL GOOLDEN Surgeon at Bridgnorth in Shropshire.*

I Cannot help not only expressing my own pleasure and satisfaction, but must also congratulate every professor of physic and surgery, upon the laudable attempt of your society, in their endeavours to improve medical knowledge, by their annual essays and observations; A work which I have long desired, and thought wanting in our dominions, and do think it a duty incumbent on every professor of physic, or any branch of it (in these countries especially) to communicate to them any remarkable observation that may fall under their cognizance, and may tend to promote so useful a design.

Since that valuable medicine the *cortex peruvianus* has been so successfully given in mortifications from an internal cause, by Mr Rushworth, Mr Amyand, Mr Bradley, Mr Douglas, and Mr Shipton; a remarkable instance of its success, in a more than parallel case, I had lately the pleasure of observing in a patient of mine. I have not the vanity to think that what I can say will add any thing to its credit; but it may be a means to make its virtues in such direful disorders more generally known, and help to confirm what the above gentlemen, and especially Mr Douglas and Mr Shipton, have so ingeniously said of it.

January

January 8. 1733-4.

Samuel Lewis, aged 76, of a pale complexion, and choleric constitution, a lusty, and seemingly a very healthy man, having but little sickness from his youth, shewed me an inflammation of his left leg, extending from an issue he had below his knee, down to his ankle, and all round his leg, partaking of an erysipelas and oedema. I threw out the pea from his issue, and endeavoured, by discutient fomentations, embrocations, and cataplasms, with bleeding and lenient purges, to mitigate the inflammation; but to no purpose; for I found it tending very fast to a gangrene. His leg, from an intense red colour, turned livid, black blisters arose, &c. I would have scarified it, but was not permitted.

On the 13th day, the tumor was sunk, his leg black and dry, his pulse quick, with frequent intermissions, his countenance wild, his tongue hard, parched, and dry. He would not permit the necessary incisions to be made. With the consent of Dr Anthony Weaver, a gentleman of great charity, humanity, and learning, I prepared eight of the following draughts.

R. Cort. peruv. opt. pulv. drach. sem. aq. cerasor. nigr. unc. i. sem. syrup. croci unc. sem.

M. One of which I gave him about noon, and ordered that he should continue to take one every four hours.

14. About ten this morning, by which time he had taken three drachms of the bark, I found his tongue moist, his countenance not so wild;

and

and examining his leg, found it impostsomated, from a little below the superior tubercle of the tibia down to the small of his leg, a little above which I saw a small aperture, with a little matter ouzing from it. I told him he stood a very fair chance for his life, if he would submit to the suitable means. With his allowance I immediately entered the probe-point of my scissars at the aperture, and cut upwards as far as it was hollow; then turned them, and cut downwards as far as the cavity ran, and discharged between three and four ounces of a well digested pus, and, after fomenting very well with a decoction of the warm plants in a strong lixivium of woodashes, *sal ammon. com. & spir. vin. camphor.* (which I had used from the time I suspected it would mortify) I dressed the incision with *ung. basilic. liniment. arcaia a. part. æq.* spread upon a dossil dipt in hot *ol. terebinth.* with a cataplasm of *oat meal, flor. centaur. chamamel. a. a. p. æ.* with the fomentation; and *ol. chamamel.* over all. He found an agreeable warmth about his leg after the dressings were applied.

15. I found him very chearful, and discovered a large sinus betwixt the *solæus* and *gastrocnemius intern.* I laid it open, and discharged about the same quantity of well-digested matter as yesterday. There was a very large slough in the former incision, which I cut off, and dressed as before.

16. He had been very restless all night, his pulse irregular, his tongue rough and dry, with flushings in his cheeks. Inquiring if he had taken his draughts regularly, I was told he

he had not, through the attendant's drowsiness. After reprimanding them for that neglect, and cautioning him about it for the future, I opened his leg, and found the discharge large, a fungous flesh rising in the first incision, which I sprinkled with *precipit. rubr.* and dressed as before; and, by reason he had not a stool since the 14th, I ordered him a common clyster, which brought away some hardened excrements. At night his heat and flushings were not so great, and his tongue was moister.

18. Being wearied of his draughts, ordered thus,

Rx. Cort. peruv. opt. pulv. unc. sem. confec. alkerm. unc. i. M. divid. in bol. viii. cap. unam quarta quaque hora superbibend. cochlear. iii. julap. sequentis.

Rx. Aq. lact. cerasor. nigr. a. unc. iv. rut. unc. sem. epidem. unc. ii. tinct. croc. in aq. theriac. fact. unc. i. confect. alkerm. unc. ii. syrup. cariophyll. unc. ii. M.

I observed matter lodged in the *gastrocnem. intern.* almost to the back of the leg; I opened it in the most depending part, but had not the discharge I expected.

21. Compresses and bandage were applied to unite that cavity, and prevent the matter from lodging in it.

22. A sinus running towards the small of his leg opened.

23. He complained of a pain in his side, and had a restless night; I dressed the ulcers only with dry lint; the cavity above-mentioned inclined to unite.

24. He

24. He was very much dejected, but I could not apprehend the reason of it; every thing appeared in good order.

25. He shewed me a swelling in his groin, with great hardness and inflammation down the fore and inside of his left thigh, extending to a pretty large and insensible tumor, a little above his knee, which he found gradually to increase since the 15th instant, but did not speak of it before, lest he should (as he expressed it) be cut there. I applied an emollient plaister over it, was apprehensive he would have a very large abscess, which would exhaust him. There was very little discharge from his leg.

30. To this day, his fever increased, with an irregular pulse, great drought, dry tongue, &c.; notwithstanding he continued the use of the draught or bolus as before, very little discharge from his leg; the ulcer appearing livid, I fomented well, and applied the warm digestive as above. The swelling in his groin very much increased; the inflammation decreasing, I felt matter to fluctuate, but deep; the tumor not very painful. Not having a stool for several days past, I gave him a lenient purge, by which he had a very large stool of black and very foetid excrements.

31. The hard insensible tumor above his knee of a livid colour, and that in his groin rising towards a point near the inguen, inclining to the inside.

1. 2. February, Instead of matter, there was a discharge of clotted blood from his leg; I dressed it with the warm digestive.

3. The pus was laudable, the tumor in his groin

groin considerably raised; he took a lenient purge, which gave him one stool, not having one since the 30th ult.

11. To this day, his fever continued, but not in any great degree, and his pulse irregular; a white pustule appeared upon the most prominent part of the large tumor in the inguen, which I cut, and then, entering the point of my probe-scissars, cut about an inch in length near his groin; well digested matter gushed out, as from a cock, and in as full a stream, sometimes streaked with blood. I took thence at least *℥* iii. His leg begins to cicatrize.

12. A large discharge about the bed from the last incision, and a large quantity of matter that fell below the orifice; yet, in the cavity on the inside of the thigh, I applied a caustic on the lowest part, and discharged thence about *lib. sem.* I also opened that tumor near his knee, and discharged *unc. i.* of well digested pus.

19. I opened another sinus on the inside of his leg, and discharged thence only several clots of blood. From this time the discharge from his thigh gradually lessened; that very large sinus united, by means of compress and bandage; his fever left him, and he did not use his medicine since the 14th instant, in which time, in draughts and boluses, he had taken between *unc. x.* and *unc. xii* of the cortex, which, being continued so long and regularly, I believe assisted nature to expel her enemy in that very large abscess in his thigh, which otherwise might, notwithstanding the mortification was
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stopt in his leg, have seized it again, or have fallen upon some more noble part, and occasioned his death; after which I made him a decoction of the most agreeable bitters, by which means he recovered a good appetite; and in a short time was able, with a little assistance, to walk down stairs, and any where else in the house with a staff only; and, on the 25th of March, he walked to my house to be dressed, which is near a quarter of a mile; and about a week afterwards went to his work as usual, (which is mending shoes) his leg giving him very little disturbance: In the day it swells considerably, but, when he rises in the morning, is of its natural size, for which I ordered a laced stocking. His thigh is strong and firmly cicatrized, as also is his leg; and the man enjoys good health, and is every way as fit for his work as he was before his illness.

VI. *A mortification cured by the Peruvian Bark;*
by Mr JOHN PAISLEY, Surgeon in Glasgow.

Gentlemen,

THE good effects of your generous concern for the welfare of mankind are now conspicuous in the many useful observations and essays that have been communicated to the public in your collections, which, without this opportunity, would probably never have seen the light. At the same time, I must think we are also greatly indebted to you for the judicious abridgment of the most considerable improve-

ments and discoveries in physick made through Europe, which most private persons remain entirely ignorant of, or it is very late before they are informed of them. It is owing to your second volume that the specific virtue of the *cortex Peruvianus*, in curing mortifications, was known early enough here to be put in practice in the following case.

A surgeon of this place, of a very bad scorbutic habit of body, about forty years of age, had a little pimple on the middle of the underlip, which his barber cut the top from in shaving him on Saturday the 9th of February last. The following evening, upon going out to the cold air, the pimple swelled, and turned hard, with an inflammation all round it; which increasing the Monday following, he applied an antiphlogistic fomentation with spirit of wine camphorated. Notwithstanding the frequent use of these for four or five days following, and his being twice bled, the inflammation, hardness, and swelling, increased considerably, extending itself to the angles of his mouth, and some way along the cheeks, and all around the chin, with great pain, and with vast disorder through his whole body.

On Friday the 15th, at eleven at night, a small black spot, about the bigness of a her-ring scale, appeared (not where the wound was, but) on the middle of the red part of the lip, which spread so fast, that, by eleven next forenoon, it covered near one half of his lip that then began to stand out very much, when a consultation of almost all the physicians and surgeons in town was called,
who

who advised the continuation of the fomentation and spirits as before, and a decoction of the woods. For two or three hours, the mortification continued to spread, till it had covered almost his whole lip, reaching inwards and downwards to the gums, the hardness and swelling of the neighbouring parts increasing. Upon this, he was advised to try the powder of the *cortex peruvianus*, half a drachm for a dose. He took the first dose betwixt three and four o'clock afternoon, and his lip was dressed at ten at night, when the mortification did not appear to be increasing, at least the increase was very inconsiderable: He then took another dose of the bark. Towards the morning of the 17th, his lip was again fomented, and he took a third dose of the cortex. At ten of the forenoon, I dressed it, and found the mortification had made no further progress since last night. At night, I dressed it again, and then, for the first time, observed something like an appearance of suppuration at the place where the wound or rather pimple was, but none at all on the mortified part. That night he took another dose of the cortex, and continued to take two doses, one in the morning, and another in the evening for two weeks.

The fomentation and spirits being applied twice a day, and a little emulsion given him for drink, without any other medicine than the cortex, the suppuration succeeded well in the mortified parts on the third day after he began to take that medicine; upon which proper digestives and other dressings were applied.

plied. The sloughs cast off very well, the hardness and swelling went away, and in 12 or 15 days the lip healed up, though with a considerable contraction by the great loss of substance.

In very cold air, he still feels a pain in his lip: This, I am apt to believe, does not so much proceed from the callus, as from his lip pressing upon the fore teeth which are very rough and loose; and which it does more especially when he attempts to speak, by the lip being so much contracted.

I have read this account to the patient, and had his approbation of my relation of the facts, which my attendance on him all the time of this disorder gave me sufficient opportunity to observe.

VII. *Remarks on Chalybeate Waters; by ALEX. MONRO Professor of Anatomy in the University of Edinburgh, and F. R. S.*

THE ingenious account of several steel-waters in some of our northern counties, given by my worthy friend Dr Thomson, physician at Montrose, [See *Art. 6.* of our *2d Vol.*] raised a desire in me to be informed of the most remarkable mineral waters of that kind which are to be met with so frequently all through Scotland; and, as my view was only to know so much of them as was necessary in practice, I proposed to discover, by my friends and correspondents, what their real or comparative strength was, and how well they would carry and preserve without losing their virtues, where-
by

by physicians might judge which of them was most proper in the various diseases and circumstances of patients, and which must be drunk at the fountain, or would serve as well when kept.

It was necessary in such an inquiry that there should be some general method by which all the trials should be made, otherwise there would be an insuperable difficulty in making the comparison of them. The writers on this subject have contented themselves with telling us, that such waters strike a red, purple, violet, or black colours, when galls or such other astringents are mixed with them, and that this change of colour is a sure mark of a chalybeate water; and some have said, that the deepest colour shews the greater proportion of steel. To satisfy myself of the truth of this, I dissolved artificial *sal martis* in a small quantity of fountain water; and then, dropping more or fewer guts of this solution into a given quantity of common water, I found that, by the mixture of the tincture of galls, I could form all the different colours mentioned, the larger quantity of the solution always requiring the greater number of drops of the tincture to bring it to all the colour it would take, and that being as constantly deeper than the others, where fewer drops of each had been employed. If words could express the numerous degrees of colours between the pale-red and the black, the lightest and deepest of these mentioned, the simple experiment of bringing steel-waters up to the deepest colour they could strike with galls, might be sufficient to determine

mine the different proportions of steel in each; but, as the variety of colours cannot be expressed in words, and I wished to come to the knowledge of nearly the real quantity of steel contained in any given quantity of each water, there was a necessity of having some general standard to which all might be brought. To obtain this, I observed the quantity of steel employed in preparing *sal martis*, saw how much salt was got, what the residuum of earthy parts was, made a small allowance for some evaporating during the effervescence of the *limatura martis* with the oil of vitriol, and concluded that the proportion of steel in the artificial salt or vitriol of iron, was very little more than a third part. Next, I dissolved a certain quantity of this *sal martis* in fountain water, weighed the powder that precipitated from it, weighed the whole solution, and then, putting some of it into a small glass, I dropped it *guttatim* into another glass, counterpoised exactly in a scale, till I saw how many drops of this liquor weighed two drachms; after which, by common arithmetic, it is easy to know how much salt, and consequently very near how much steel it contained in any given number of such guts. To save my friends the trouble of making such solution, I prepared 20 ounces, which contained an ounce of the *vitriolum martis*, except a scruple which was precipitated, and 142 gutts of this solution weighed two drachms; therefore every such gutt contained $\frac{1}{142}$ of a grain of salt, or $\frac{1}{9}$ of a grain of steel. The difference of the bulk of the drops let fall from different glasses should not,

not, you see, at this rate, make a very considerable error ; but, to prevent this as much as I could, I chose all the glasses as near to the shape, size, and thickness of the lips of the one I first used, as I could get them.

To make a comparison then of any chalybeate water with this solution, into a determined quantity of such water, pour drop after drop, of a strong clear tincture of galls, allowing a sufficient time between each drop, for its having its full effect, till it is observed that the addition of more tincture makes no change on the colour of the water ; and, to make sure of the number of the guts of the tincture that are requisite, let the experiment be repeated several times. Then having the same quantity of common water as was employed of the mineral water, in a glass of the same dimensions, thickness, and transparency, with that made use of in the preceding trials, pour into it the number of drops, discovered by them, of the same tincture of galls, and mix them well ; after which, in the same cautious manner, drop in the solution *guttatim* till their colour is the same with that of the mineral water. When once the quantity of solution, equal to the contents of the spaw, is known, pour a due proportion of it into common water, and let several people examine whether the taste of it is not the same with what the mineral water has. I have thus made fountain water so like to several chalybeate waters, that none could distinguish them.

I prefer tincture of galls to their substance for making the foregoing experiments, because
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it produces its effect much sooner and more equally than the powder, and a less proportion of the virtue of the galls can be added at once; which, from an observation communicated to me by Dr John Taylor, physician here, and verified by me afterwards in several steel-waters, and in common water impregnated with *sal martis*, would seem very necessary to be regarded; for, if too large a proportion of galls is at once poured into such waters, for example, if 60 or 100 drops of a tincture of gall is thrown into a water that requires only 8 or nine to bring it to its deepest colour, it will be so far from making it strike the colour stronger, or sooner, that, for several hours after, no change will be observed on the water; and at last it gradually becomes of a deep sea-green colour, instead of the purple or violet it would otherwise have turned into. It is not impossible that good chalybeat waters have been sometimes condemned as containing no such mineral, or of being impregnated with copper by a mixture of too much galls at random.

Recent tincture of galls is certainly preferable to that which is long kept; but, I can assure you, that, after I had kept such tincture till it was covered with a thick moss, or was mouldy, as we call it, a-top, and had a viscous thick sediment, it still produced the common effects on steel-waters.

I would propose, that the comparison above described should be made with fresh water at the steel springs in different seasons, and should be repeated once a week with water that is right put up in bottles, well corked and sealed, in
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these different seasons, till it becomes vapid by keeping; by which the proper seasons for bottling the mineral waters, and the time each will preserve, can be much more exactly known than it is possible to determine by the smell and taste of them.

It will be also requisite to observe what time it is before the galls strike the colour fully after they are put in, and to remark how long this colour remains in an open glass; for it appears reasonable enough to think, with the French academist Mr Geoffroy *, that more time will be necessary to strike the colour fully, but that it will remain longer in such mineral waters where the steel is most intimately dissolved in, and blended with the other principles.

To make the account of the spaws compleat, their other contents ought to be sought after, by mixing different substances with them, remarking the changes they undergo in smell, colour, &c. by keeping, and by extracting their salts and earths after evaporation.

By these means it is, that I was in hope to have furnished you with a pretty compleat list of the most remarkable spaws in this country, with the comparative strength of each, and the time they kept; but, being disappointed of several I expected, while informations of others I had not heard of are often brought me, I shall reserve what I have received, till I can present you a more full account, which your publication of this invitation will probably

* Memoires de l'Acad. de Sciences, 1724.

bably soon enlarge, by acquainting the gentlemen who have the opportunity of examining such waters, of the common method they should all take in their inquiries, and to whom they may address their discoveries. In the mean time, allow me here to annex some experiments, which thinking on this subject led me to make.

I had observed, as above, a strong resemblance between our steel-waters and common water in which a small quantity of *sal martis* had been dissolved; but the natural spaw waters, when exposed to the open air, very soon lost their chalybeat taste, and would not strike a purple colour with galls: When exposed to heat, their virtues were much sooner lost, and in the closest vessels they, in no long time, became vapid: Whereas *sal martis* bears heat, and being exposed to the air, without perceptibly losing any thing. I suspected this difference might in part depend upon the smaller proportion of the vitriolic principles in the chalybeat waters, and some change they might thereby undergo in the water; and, therefore, having added to some bottles of water as much *sal martis* as had been found to make it of the same taste, and to strike the same colour with chalybeat waters, I corked some carefully up, others I put bad corks into, and a third sort I left open. The water in these last lost its taste and virtues in about a fortnight, with little other change than becoming a little muddy, and having a saffron-coloured powder at bottom. The second kind

kept

kept some time longer, but had a little of a stinking smell before it became vapid. The water that was carefully corked and rosined, kept well, but acquired a pretty strong smell of rotten eggs, exactly like to what several spaws had when kept; and, when the bottle was left open, the stinking smell went soon off, and soon after the chalybeat virtues were not to be observed, and the bottom of the bottle was covered with the saffron-coloured powder, which is generally to be seen also in bottles where natural chalybeat waters have been kept any time. So far therefore the difference between the natural and artificial steel waters seemed only to be in the greater volatility of the natural ones.

Several of our own chalybeat waters, and the two foreign ones of greatest reputation, Spaw and Piermont, seemed to me an exception to the resemblance I had found between the diluted solution of *sal martis* and the natural steel waters; for the colour they strike with galls is very faint, while their taste and other effects are remarkably strong. This put me on trying to find some artificial way of imitating them: For this end, I mixed filings of iron, oil of vitriol, and water in a florence bottle, which I laid on its side, and immediately fitted another to it, in which I had put some fountain water. The fumes that rose upon the effervescence of the oil of vitriol with the steel, came over into the other glass. After the violent effervescence was over, I took away the glass with the water, which was quite limpid, but had a strong empyreumatic

smell; its taste was pungent at first upon the tongue, and then the acidulous taste prevailed. When tincture of galls was mixed with it, it became of a red-purple but faint colour; which held a great many days without any observable precipitation. Next morning, the empyreuma of the remaining water was gone, and it had a very agreeable brisk spaw taste: In less than a day after, this also went off; a small quantity of the saffron-powder was fallen to the bottom of the glass, and the galls had no effect on the water.

On a suspicion that this artificial water would give evident signs of acidity, whereas the solution of *sal martis*, and some of the spaws that strike a black colour, rather appear alkaline, by changing syrup of violets and clove-july-flowers into a green colour, I mixed it with these syrups, without changing the one into red, or heightening the colour of the other. It is true, it did not make them green, neither do the more spirituous spaws.

The success of this experiment, which was beyond my expectation, led me to try if I could not discover what it was that evaporated, and what was precipitated in these waters. I suspected the menstruum to fly off, and the steel to be left behind. To know then if this saffron-coloured residuum was steel, I poured oil of vitriol on it, which made no remarkable effervescence: I then added water, and let them stand two days, when some of the powder seemed to be dissolved: Upon mixing tincture of galls with it, the colour did not change; but, upon further addition of spirit

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of hartshorn, a great effervescence followed, and a sudden coagulum of a deep red purple substance was made. This change of colour I at first imagined to depend on the residuum; but, upon mixing the other ingredients without any residuum, the same coagulum was formed; and therefore, if the powder precipitated from the mineral water is the particles that composed iron, they are greatly changed. The resemblance of the rust of iron to this powder, made me try how these experiments would hold with it; and they came out in the very same manner: Nor would rust, or the powder, when suspended in water, strike any colour with galls, though the *crocus martis* of the shops did.

What flies off in the evaporation is evidently the menstruum, in which the iron particles were dissolved, which carries away some of the principles of the iron with it. From seeing the effects of acids on iron, observing the effects of that almost universal menstruum of the air upon this metal, and finding vitriol of iron naturally formed, we have reason to judge that the menstruum of the iron particles in chalybeat waters is also an acid. And, considering how much sulphur enters into the composition of iron, what a strong stinking smell (which can only depend on sulphur) mineral waters have before they turn vapid, and how much the sulphur of iron is destroyed before the iron turns into rust, which again resembles the powder precipitated in these waters; all this would persuade us, that the menstruum carries the

fulphur along with it, and leaves only the earthy parts, with a small proportion of sulphur.

May not steel-waters be impregnated with common gross vitriol, or with the more subtile fumes of iron dissolved in the natural menstruum, or with both in different proportions?

Will not the less volatility of some chalybeate waters, and the quicker precipitation of their irony particles, after the mixture of galls, shew the sulphur not to be so much freed from the earthy particles, as in others that precipitate more slowly, and are more volatile?

Will not the common observation of air generated in all effervescencies, fermentations, putrefactions, and where-ever a considerable change is produced in the composition of bodies, account for the quantity of elastic air, observed in chalybeate waters, in the more spirituous when recent, in others when the putrid smell shews the sulphur to be more disengaged?

Are not the different kinds of steel-waters to be prescribed, according as there is occasion for a subtile penetrating sulphureous spirit to pervade the smallest vessels, or according to the quantity of an absorbent astringent earth that is required to be joined with this spirit?

Where different spaws are not to be had, may not the same water be made to answer each of these intentions, according to its being more or less kept, or exposed to the air or heat?

VIII. *An Inquiry into the Mineral Principles of Montrose Water*; by ALEXANDER THOMSON, M. D. *Physician at Montrose.*

AFTER all the inquiries made into the contents and natural appearances of mineral springs in Germany, France, and England, by men most accomplished, and means promising the best success, the subject seems yet much in the dark, when all appearances are justly weighed.

Of the latest inquirers into the English waters is Dr Short, and after him Dr Shaw has examined the Scarborough water particularly. They have entered more into the subject than most others before them, and offered fairer at a discovery of their virtues from their real principles; yet all the learning of the first, and accuracy of the other, have still left the subject involved in variety of doubts, of which I shall only mention a few.

That spaws strained through mineral bodies, dissolvable in water, may not, and at times actually do not partake of all, but only of some of them, as Dr Short hath observed of the allum stone in the clefts of the rock through which the Scarborough water strains; and that yet the water gives no vestige of allum, upon all his trials.

From the same instance of allum, he hath, I think, observed something more mysterious in the composition of mineral waters, to wit, That, in the congeries of mineral bodies through which they flow, two of different

natures, as allum and sulphur, may be so involved, that the one apparent as allum, gives, upon trial, a demonstration of the presence of the other, whereof nothing appeared before; as in the solution of the above-named allum-stone, both the sludge, or undissolved dreg of the stone, and also the sediment of its solution, give an intolerable foetor, as strong as that of the Harrogate sulphurous well, and, in a red-hot iron spoon, throws out a blue flame, whereby sulphur is known to be distinguished.

That the purple tincture of steel spaws, with galls and such like, should be owing to the vitriol of chalybeat waters, and notwithstanding what gives such tincture should inevitably fly off less or more, or altogether, an hour or two after drawing, or sooner by several degrees of heat, the highest within the boiling degree, when nothing in vitriol that gives such tincture hath been observed to fly off in that manner.

That the medical effects of mineral waters in general, in cases of the spirits or animal faculties affected, should be generally observed, and particularly by Dr Short, to be best when drunk at the fountain; and yet that Dr Shaw should find, that the salt of Scarborough water, got by evaporation, cheared the spirits, and in general gives life and fresh powers to nature, as much as the water when drunk fresh at the fountain.

That a mineral water should be 17 or 18 grains a pint heavier when carried to a small distance, than when immediately drawn, as Dr Short hath observed of Scarborough water,

ter, and this should be owing to a more dilated air, or a very subtile mineral spirit; and yet no air for being more dilated is less a body, neither does any mineral spirit, for being subtile, become no body, and therefore not ponderous. Bodies differ from bodies in their specific gravities; but no subtilty or dilatation hinders every body from gravitating: And therefore, whatever it be that evaporates from the water, it should, by all that is known of bodies universally, rather diminish than augment their weight by its absence. Nor would there be less difficulty to conceive a reason why the weight of the water thus increased in so short a time, should increase no more a day, a month, a year after. All this notwithstanding, the Doctor hath likewise observed, that the chalybeat Scarborough water weighs still heavier the more it is freighted with this mineral spirit.

The general uncertainty wherein the greatest naturalists, as Mr Boyle and Mr Hoffman, have put or left us, on the subject of mineral springs; and the more particular difficulties I have now chose out from among several others, which, for brevity, I omit, wherein the two learned physicians I have named, after their more accurate discoveries, have left us yet involved; and the laws of inquiry, proposed by the latest of them, requiring an age, or it may be ages, rather than the endeavours of any one, or of a few men: These considerations, I say, are a sufficient caution to me, not to be too decisive in a matter still so obscure.

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The MONTROSE-WELL is situate in the richer sort of our soil; the upper lay being of a blackish or moss-colour; the lower, to about three feet depth from the surface, lays interchanged perpendicular, one of soft clay, the other of sand, soft, and as it were fattish to the touch; through which last, at the above depth, the springs drill out horizontal to the surface in several parts.

The water, when new taken up, is of a whitish colour, above that of ordinary spring-water; the taste soft, and faintly discovering the mineral quality. I compared the weight of this water with two of the most noted of our steel springs, and one of fine ordinary spring-water, in a balance that was turned by one grain, and could observe scarce any difference of weight in any of them. This difficulty I cannot well account for.

I began my trials on this water with those commonly made with steel springs, but they did not answer; and I suspected its purgative quality on the drinkers might be owing to its being generally drunk very muddy, by people's thronging on it, till it was built about. A few days after, by an accident, I began to conceive it to be of a different nature from the steel springs. A phial-glass into which several parcels of the water had been thrown at random, with several things, as galls, rose-buds, and green tea, being broken by accident at night; in the morning, I perceived a white milky substance covering the floor where the glass was, and about it a scurf variegated green and other colours, much like
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these of a peacock's tail. These led me into a suspicion of something sulphureous in the water.

I dropped some gutts of *ol. tart. per deliq.* into a glass of the water, and there presently formed at the bottom of the glass a white light equable cloud, such perfectly as appeareth ordinarily in the urine of people in fevers, at the time when they have a favourable crisis, or immediately after it, the one and other waving with a plain equable surface, by inclining the glass to and again, only that of our water was of the purest white. As this appearance in such urine gives, as I imagine, plain notice of the morbid sulphureous parts of the blood now perfectly concocted, and so washed off by the urine, as by the other strainers of the body, the analogy seemed to me natural betwixt the two appearances in the urine and in our water.

To try this further, I made *sulphur solubile cum alkali fixo* after Boerhaave's way, four parts sulphur to a fifth *sal tart.* This dissolved in water, and *ol. tart.* dropped on it, gave a cloud, but less discernible than what appeared in our water, which I imputed to the yellow tincture of the solution not reflecting the rays fully: And accordingly a day after the *sulphur solubile* having become moister by the air it imbibed, and turning whiter, I dissolved it again, pouring on water till the solution became of the colour of our water when new drawn, and the *ol. tart.* affused gave conspicuously the same manner of cloud with that in the mineral water, so that the one could scarce
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or not at all be distinguished from the other.

The success of these experiments, compared with the above accident, made me incline to believe that these clouds were lac of sulphur. To make trial of this further, I would see what a strong astringent salt would do with the water of our well; I chose allum, and, weighing a small piece of it, I put it whole among a glass of the water, and, by constricting its pores, (as I imagined from the allum's having lost none of its weight when taken out of the water,) it quickly formed a white coagulum, which, by mixing some drops of *ol. tart.* presently falling to the bottom, I poured off the water, and evaporated what remained by the heat of the sun, into a concrete mass, of a soft taste, and with nothing saltish therein.

For trial whether the effects of the alkali salt might be the same, or near so, on sulphurs universally, whether volatile or fixed, I dropt some guts of *ol. tart.* on common spirit of wine, and presently had an equable cloud formed towards the bottom of the glass, and of higher colour than the liquid above. Just so when it was infused on *ol. oliv.* it formed a cloud or lac of an equable surface to the bottom; for the oil above poured easily off, the conspicuously white lac remained. The same succeeded with the solution of *sal tart.* among water poured on these liquors.

As I here only consider the analogy of things, I have not to do with what is called the *rationale* of them, or to account how the alkali, which dissolves sulphur, or renders it soluble,

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luble, should in such manner appear to precipitate it, by separation of parts, an effect rather belonging to acids, as in making the *lac sulphuris*: Neither will the narrow bounds I am confined to allow me to enter on such inquiry; and the rather that it will further appear, that chymists are yet much in the dark concerning the phænomena of these famed salts, or what are generally reputed such.

I went on next to try what appearances spirits, both the alkaline and acid, could make on our water.

By spirit of hartshorn, there quickly rose a-top of the glass an equable cloud all over the water, resembling much the colour of old brandy, which, in about half an hour vanishing, left the water more milkish, and as it were troubled. This seemed to answer well enough to the former experiments.

Spirit of nitre turned its native milky colour into a fine light-blue, from the top to below the middle of the glass, and, as it stood longer, the colour advanced further downwards.

Spirit of vitriol changed the whole body of the water into a faint purple, or dilute pink colour.

As I could make nothing of the two last trials, I tried next solution of sublimated corrosive; and by some drops thereof on our water, the same manner of cloud began to form immediately, but formed out more gradually.

The same solution, on solution of *sulphur cum*

cum alkali fixo, made presently a coagulum, but no equable cloud.

By these, with the two former, it appears that the alkali prevails in the salt of our water: But this can determine nothing concerning the species of the salt; as it will appear, that even the most reputed acids of the fossil salts prove no less alkaline upon trials.

But what comes most to my present purpose, is, That the analogy hereby appears of the effects of the strongest alkalies and strongest acids, both agreeing in forming an equable cloud in our water. And I conceive the coagulum formed in the solution of *sulphur cum alkali fixo*, to have arisen from the cloud being confused by the mercury being more disengaged of its acid menstruum, by a stronger alkali than is the salt of our well, and thereby precipitating in greater quantity.

I found by the above trials on Scarborough water, the analogy and differences following betwixt it and our water.

Ol. tart. affused on it, formed immediately a thick milky cloud standing equable in a minute or thereabout.

Solution of sublimate corrosive on the same water formed immediately a thin white cloud, rising to the middle of the glass.

Solution of sublimate corrosive on our water gives just such a cloud as *ol. tart.* on Scarborough water; and, when it begins to diffuse itself, the affusion of a little more of the solution makes it become again more conspicuous.

Solution of *sal tart.* forms the cloud in Scarborough

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Scarborough and our water alike ; with this difference, that in the Scarborough it formed more immediately, in our water more gradually, but standing longer without diffusing.

I made a lac of the Scarborough water in the same manner, and answering to the same sensible qualities as that made of our water.

The lac may be made of both also, by a piece of a gall entire, or any such like astringent, and *sal. tart.* but not so pure.

Powder of galls mixed with both, gradually turned both into a dusky green, with foliages suspended through the waters, in irregular planes.

These, I think, are as near resemblances as are readily to be found betwixt one water of the mineral kind, and another ; and any variety that appears may be owing to some variety in the salts of the one, not so much, or at all, in their nature, as in their combination, or in some difference of proportion. To know what account to make of these, and whether any salt by itself, or in combination with others, could come up to the above appearances, I made the following trials.

I made the experiments, as above, on each of the fossil salts, and also on sea-salt dissolved in common fountain water, first separately, then on their various combinations, as I could conjecture they might most probably prove, or be the contents of our water, but found none except the following come up to such purpose.

Ol. Tart. on solution of nitre makes no change of consequence ; but, on solutions of nitre and of sea salt mixed, formed a cloud

to the bottom; but solution of sublimate corrosive on the above solutions mixed made no change, though remaining a considerable time in the glass.

Solution of sublimate corrosive on solution of nitre made at first no change; but some minutes after, formed a cloud, as by *ol. tartar.* on solution of sulphur with the fixed alkali, and on our water.

Mixture of solutions of allum, nitre, and sea-salt, made a troubled white.

Ol. tart. assused gave some appearance of a cloud, not equable, but interspersed as in precipitations, and swimming above the bottom.

Solution of sublimate corrosive on the above solution, does nothing further than change the same into bluish.

Ol. tart. on solution of sea-salt, for a considerable time made no change; but then falling down troubled, and making it as it were muddy to the bottom, it gave some faint appearance of a cloud, but not so equable.

Ol. tart. on sea-water made immediately a white precipitate, then quickly formed a thick cloud, which some time after precipitated an equable cloud.

Spirit of hartshorn on sea-water formed quickly an equable cloud or suspension a-top of the glass, about the breadth of a small straw, much resembling that made by the same spirit assused in our water: But I could observe nothing of that whiteness nor curdling that Dr Short observed to be made by this spirit and *ol. tart.* on sea water. I at first suspected

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suspected this suspension a-top of the glass by affusion of the spirit, to be only the spirit a little tinged, getting a-top, and tincturing both our water and the sea-water. To make sure whether it was so, I affused a little of the spirit on good spring-water, and waiting some time, I found no change, and thereby perceived a further improvement of the above analogy, which I shall now state.

By the trials above, it appears that no salts come up to any analogy with our water and that of Scarborough, except nitre and sea-salt; but especially that of the sea, without evaporation, comes nearest. Yet none of them, either by themselves, or in their combinations, come up to them in the trials both with acids and alkalies; whereby it appears that the specific qualities of these waters are not confined to either or both these salts or the earths they lodge in. As on the other hand, these salts being both sulphureous, thereby they the better correspond to a further sulphureous principle in these waters, answering more perfectly in the analogy above stated with sulphur itself. The inflammability of nitre proves its sulphureous kind; and the difference of the salt of sea water before the evaporation from prepared salt, appears plainly above, from the different effects of the fixed alkali on sea-water, and on solution of sea-salt. This difference is owing to the sulphur of the salt in the water before exhalation, as may appear from the volatile alkali casting up the suspension or cloud abovementioned, which it doth not in the solution of sea salt more than in

common spring water; at least the one was conspicuous, the other scarce or not discernible.

This agrees with, and is confirmed by Dr Short's account of this matter: For he kept sea water close corked and pitched, with a bladder tied over the neck of the bottle a month; then opening the bottle, exposed it a few days, then shut it up close again: On opening the bottle again after some time, he observed that it stank insufferably, not much short of the Harrowgate sulphur-well. The Doctor imputes this foetor to a high alkali in the salt of the sea; which I do not well understand, since no alkali, as a pure alkali, especially of the fixed sort, hath been observed to be foetid without being sulphureous, as I believe sulphur hath hitherto been observed to be the subject or substratum of all foetor; and the smell agreeing with that of the Harrowgate well, should rather have determined its quality to be of this kind: Therefore I rather would agree with the Doctor's conclusion, that it contains nitre, and ascribe accordingly the foetor from its corruption to the known sulphur of that salt; but it is not my business at present to judge in such things.

The last thing I have here to observe on this part of the analogy is, That as *ol. tart.* on solution of nitre and sea salt, gives a cloud, and solution of sublimate corrosive, on solution of nitre alone, gives also a cloud, I tried to make a lac of both, in the manner I did by our well; but I had nothing of either but a coagulum swimming through each, with-

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out any lac falling equably to the bottom: So that hitherto I have found nothing but of the sulphureous kind, which gives a cloud; neither can any thing but sulphur itself, it seems, give a lac. By a pure accident, I saw this further confirmed; one of the mixtures I made of the solutions of salts as above, particularly of allum, nitre, and sea salt, with *ol. tart.* affused, thrown at random into soap-water, an equable lac fell to the bottom, leaving the water clear above.

So far analogy hath led me, and I have kept the laws thereof the best I could, without straining beyond what nature can easily bear. Other ways have been thought on to get into the intimate principles of mineral waters, but success hath not well answered the design; nothing hath yet been found to come over the still, whereby we could have learned more for any real use, than we could have attained to without that labour. Chymical mixtures, to discover contraries by contraries, alkalies by acids, and acids by alkalies, have as little reached the composition of natural bodies, the mineral especially. All the fossil salts, which authors have contended for, as the contents of mineral waters are supposed acids, and appear so more or less to the taste; yet they have been all of them, the most acid not excepted, as vitriol and allum, found, by the trials of the learned and ingenious Mons. Tournefort in his preface to the history of plants, to go less or more on the alkaline nature; as of the most alkaline of earths, burnt limestone wants not its own acid. So that upon the whole these names teach us less

in nature or medicine than is commonly thought; whereas the way of analogy betwixt things known and things sought for, if right pursued, seems the most universal way chalked out by nature for us, in our inquiries of nature; and if it come not up to demonstration, it still comes nearest to truth of all the other methods of mens contrivance. The learned have contented themselves with this method in their inquiries into steel-springs. Galls, with infusion of iron, or its vitriol, gives a higher or lower purple: Therefore, whatever with galls strikes such a colour, is concluded to be of iron-mineral. Why may not the same way of reasoning on all mineral springs be allowed?

To know the more adequately to what species of salt or salts that of our well and of Scarborough might be reduced by further trials on them, I was at the pains to compare and mark down the agreement or disagreement in every one of the fossil salts, also in sea salt, as they were tried by the learned and ingenious author above named; and from his trials, compared with those above, and others to be yet named, on the decoctions of these waters, I found the agreement of their salts with nitre and sea salt much nearer than with any other; though there is none of these salts but what, by some trial of the same author, falls in more or less into the quality of the other. The particulars I would have marked here, but that they would have exceeded the just bounds I ought to confine myself to: And, if any have the curiosity, he may satisfy himself, by comparing the trials with Mr Tournefort's book.

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By the experiments also of the same accurate gentleman on the natrum of the Levant, compared with these on nitre and sea salt, and all three compared with his chief inquiry concerning the natural salt of the earth, it appears, that the natrum and natural salt answer almost the same trials, and that the nitre and sea salt answer to both more than any other salt; that is, nitre and sea salt appear to make up the greatest part of this universal salt. And, if to this we add his discoveries concerning the natural sulphur of the earth, we may hence probably have the more natural account of such mineral springs formed by the spring falling in its way through the crannies of the earth, into a congeries or beds of such mineral substances gathered together, as certainly it is from a greater congeries of mineral particles under or above the surface of the earth, serving as embryos of concrete minerals, that such minerals are formed. I am glad that Dr Short seems to account for the comparative strength or weakness of one and the same mineral waters at different times, as well as of two springs of the same kind, from some such thought as this. It would also hence seem that we need not be over anxious in finding out, or searching after this or that concrete mineral in the neighbourhood of any mineral springs, for determining its nature or contents; though still the presumption may be strong in such case, that the one qualifies or assists the other. But, after all, this presumption does not come up to certainty; for the same learned author hath observed,

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observed, that, notwithstanding the allum-stone in the clefts of the rock through which the Scarborough water flows, yet it hath nothing of allum in it.

I thought it best to finish this analogical inquiry into our water, and to give it entire, and in its native condition, before I should enter on examination of the contents got by evaporation of the water.

I caused to dig as deep as could be well reached, with a small shovel, along the bottom of the sand-lays, through which the small springs of the water run, separating some pounds of the sand from the adjacent clay: This I boiled leisurely in about four English quarts of the water to a half; then, pouring off the water, I strained it through grey paper. This I put to a slow evaporation on the fire, observing if any pellicule appeared; but I saw none. After drawing off the remaining water with bits of grey paper, and evaporating the moisture at the sun, the residuum came to about five grains of a black substance, scintillant in thin foliages, and of a fattish taste. I put this in two drachms of spirit of wine rectified, and had gradually a sulphureous greenish yellow tincture. I dropt a little of this into a glass of water, which shewed itself quickly a-top, of an equal surface; and, after some hours advancing downwards, its colour became more dilute to the bottom. Then I dropt on it some gutts of *ol. tartar.* which formed a cloud to the bottom, equable and waving

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waving by the motion of the glass, as above narrated.

Having poured off this tincture, I put on the residuum about the same quantity of spirit of hartshorn, which drew a black tincture resembling a bitumen by its thickness of consistence. I poured water on it, and it fell to the bottom without mixing with the water: After standing a while, it rose up again gradually, but did not incorporate with the water. Some guts of the solution of *sublimate corrosive* precipitated it, and let fall a black powder. This, after drying, I cast into a red-hot iron-spoon, and it sparkled all over; but, cast after on a live-coal, it sensibly flamed blue. I threw the remaining residuum three several times after into the red-hot iron spoon; it still sparkled as before, remained black, and its scintillant particles shewed themselves as at first. To try further what the heat of a furnace could do, I put it into a crucible placed about five minutes in the most intense heat of a goldsmith's furnace, and got returned me near one half, after it had stained the bottom of the small crucible with an indelible black colour.

I tried next, whether the lac I had made as above would prove inflammable by putting it on a fire in an iron-spoon, but I had no fusion nor flame by the heat increasing; on the contrary, when the spoon became red, my lac turned into a black cinder; but having thrown some of the lac into the spoon first made red-hot, it presently took flame, and continued so till it burnt into black ashes.

I would next try what a heat increasing leisurely would do with some *lac sulphuris* I had lately made with the *calx viva*, it did not flame in the red-hot spoon, but burnt to ashes. I tried the same with the officinal *lac sulphuris*, but neither did it give any flame till the fire was too hastily increased: But then, the spoon being taken from the fire, the flame ceased; and, the spoon being put on the fire again, the lac burnt also into ashes without flame. I tried also some sulphur of Aix in the same manner, and the success was the same, it being reduced into a red cinder, which gave no manner of flame when thrown into the red-hot spoon.

These things, it may be, are paradoxes concerning sulphur; the whole whereof is believed to be inflammable, in whatever manner it is treated on the fire.

Some time after. I tried in the red-hot spoon a lac I had made of Scarborough water; but it flamed not openly, though put on the fire; only in about a minute it shined above the brightness of a live-coal, and resembled a phosphorus. Having at the same time by me a lac I had formerly made of our water, I treated it in the same manner, and the appearance was just like to the other, both continuing in that bright state a pretty good time before they were reduced to a black cinder; whereas, when the lac of our well broke out into open flame, it quickly changed into a cinder. I tried, in the same manner, a little of the residuum of Scarborough water after

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evaporation, much resembling in colour the lac of both, and the success was just the same.

Though I think this appearance on the fire demonstrated a sulphur in the contents of both waters, I had various conjectures why the lac of our well had not answered to open flame as formerly; I afterwards thought on yet a surer way to try the inflammability of the contents of both. I caused a crucible to be made red hot in a goldsmith's furnace, then, having removed all flaming coals from the neighbourhood of the crucible, that any flame without might not confound the appearance within the crucible, I cast some of the residuum of our well into it; and ordering to push the fire by blowing the bellows, I saw the flame within the crucible gleaming, then filling the crucible, and continuing so a considerable time, the bellows still going. Then, having emptied the crucible of the residuum, I tried in the same manner the residuum of Scarborough water, and the success was the same.

In these trials, it was not possible to approach so near as to feel whether the flame smelled of sulphur or not, only it was more white, and going more on the colour of the flame of nitre in detonation. For understanding what reason there might be for such appearance, I reflected on what I had formerly observed, on a parcel of the residuum of Scarborough water, that in two or three days, by attraction of a moist air, it had augmented considerably, both in bulk and weight: Also considering a vulgar expedient to make smoking coals burn clear, by throwing
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salt upon them, I thought that the salt's impregnating the waters as above, might in great measure procure the difference. To determine this, I cast equal parts of sulphur and sea salt in powder into a red-hot crucible, whence immediately arose a white flame, going on the blue towards the end; but, at the nearest distance I could approach my nose, I could perceive little or nothing of the sulphureous steam; neither were others who stood by sensible of it. The flame ending, what remained was a grey acrid concrete. Nitre and sulphur, mixed also in equal quantities, and thrown into the crucible, gave much the same flame, with a sibilus, but no sulphureous steam perceptible by me, nor others standing by. After some time flaming, the mass went into fusion as a rosin at the bottom of the crucible.

Hereby appears that the salts attract the smook of the sulphur, imbibing it by the same mechanism as one of them attracts that of coal, and both of them the moisture of the air; and so I think the difference of flames, as above stated, may be understood.

For finding whether there might be any thing regulin or metallic in either of these waters, I mixed both residuums with one half tartar and as much nitre. I tried first that of Scarborough water, the quantity being about two drachms, which was kept ten minutes on the strongest furnace-fire in a crucible, with a cover of its own kind. After cooling, it was easily shaken out, retaining the form of the bottom of the crucible, and being white as before exposed to the fire, only interspersed with
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some blackish particles, and on the upper surface appearing porous, and the whole resembling a spongy concrete of salt and chalky earth, the taste acrid, the weight exceeding but very little the weight of the tartar mixed with it.

Before trying the residuum of our water in the same manner, it came into my thought what appearance it might make when thrown into a red-hot crucible before being put into the fire. When this was done, it began to boil immediately with sparkling, and then cast up a thick cloud of smoak with a high bituminous smell. I regretted I had not thought on doing so by the Scarborough residuum, and the rather that I had not more of it for trial; yet what is said may in a great manner supply this omission. The residuum of our water appearing in fusion on the fire, and, after it cooled, adhering so close to the crucible, that it could not be got out but in parcels by an iron bodkin; it looked like a hardened dark rosin, and had an acrid taste: Which appearances, compared with that above of the fusion of sulphur and nitre into the same manner of substance, is, I think, a good addition to the proof of our well's sulphur going more on the nitrous kind, as the appearance of the Scarborough residuum, compared with what remained after flaming of sulphur and sea-salt, may prove, that its sulphur goes more on this salt, and that both are lodged in a calcarious earth as a substratum, wherewith, as by after-trials may appear, they are both so combined, especially the sulphureous part, that the violence of fire

cannot disengage them, so that the sulphur does not always discover itself.

The decoctions of both waters, when farther carried on, may give further light into the above comparison. I boiled an English quart of each to about two ounces, and observing the progress, I found our water become still of a deeper colour, till, about six eighths being boiled in, it became like to a strong decoction of guajac wood, in the smell, taste, and colour; that of Scarborough, when boiled that far, resembling rather the colour of whey, and having the taste more briny, in respect of the resinous and mellow taste of the other.

I attempted next to find if any of the substances usually employed in trying mineral waters, when mixed with the decoctions of these waters, and, by comparing their effects with those observed upon their mixture with the fresh waters as above, would, by the difference, discover any thing further concerning the nature of the salts in the waters, particularly, whether and how far they might be accounted volatile or fixed; whereof I could discover nothing that could be relied on in our water, kept, some of it a month, some two months, in cask or in bottles, shut or open, simply corked, or with the corks laid over with pitch; as neither could I perceive any difference of smell but what was common to any spring-water, when kept a considerable time.

To extend this comparative trial a little farther, I took in two other substances than were formerly employed, to wit, syrup of violets
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and tincture of turnsole, for which last, after Monf. Tournefort's way, I made use of blue paper.

1. *Syr. viol.* turned both our water and that of Scarborough into green.

2. Blue paper dipped in our water changeth not, only the blue appeareth more intense in our water. No change at all when dipt in Scarborough water.

3. A decoction of our water changeth not by *syr. viol.* neither does the blue paper change its colour thereby. But,

4. Scarborough water evaporated to one half, becomes, by *syr. viol.* conspicuously green. Blue paper dipped in the decoction changeth not colour.

5. Solution of sublimate corrosive on decoction of our water, at first makes no change; but, some time after, troubles it into a whiter colour, then precipitates a dusky residuum.

6. Solution of *sal. tart.* on the same decoction, gives presently a precipitate equable as a cloud.

7. Solution of sublimate corrosive changeth the decoction of Scarborough water gradually into a troubled dusky grey.

8. Solution of *sal. tart.* on the same decoction, makes no notable change.

9. Infusion of galls changeth not the decoction of our water, but presently makes a coagulum in decoction of Scarborough water, falling downwards from the top.

By the first of these trials it appears, that the salts of both waters, in their native condition, goes most on the alkaline, as it appears by

Monf. Tournefort's trials, the natural falt of the earth alfo does, whatever may be generally faid of its acid nature. It feems, by the third and fourth compared, that the falt in our water is more of the volatile fort, agreeing hereby more with nitre. The fifth alfo may feem to confirm the fame; but the feventh makes this more ambiguous, and the fixth renders it yet more doubtful; and the fixth, compared with the eighth, might rather feem to give the volatility to the falt of Scarborough-water; but the ninth to reftore the fame ftill to our water. In fuch counter appearances, I tried if any thing more certain could be made out, by affufing a little common water on the remains of the refidua of both waters left in the crucibles, after trying their inflammability, as above. On thefe infufions, I made over again the fame trials; but appearances were fo little confiftent to thefe above, or to one another compared, that I found little elfe but the folly of thinking, by fuch chemical attempts, to reach nature further than conjecture reacheth; and that, as I faid before, all we can pretend to by fuch trials, is to find out by analogy the congruity or incongruity of one thing we know, with another we feek after, without pretending hereby to come at the intimate principles of things, which we have hitherto in vain been hunting after by fuch means. All that we may account certain is, that nature affords nothing pure alkali or pure acid: Or, in other terms, whatever it be of any product of nature, which is proved of the one or the other nature by one trial, may be proved alfo of the contrary nature

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ture by some other different trial. This Mons. Tournefort hath proved by multiplicity of experiments on the natural salt of the earth, on the natrum of the Levant, on sea and all fossil salts. In fine, the violence of the fire cannot quite break this make of natural bodies, so as to deprive it quite of its native texture, as he hath also proved by his trials on lime-water, that the acid hath still a being in burnt lime-stone. And I had been tempted to suspect, that all my trials of analogy failed of proving the sulphureous quality of our water, by several of the attempts to burn the residua failing, when they were exposed to the greatest violence of fire in a crucible, had I not seen the flame of the same residua in the furnace before. But what more mystery is in this, than in steel waters failing in the trials by being exposed to the least evaporation, either when open to the sun, or even in the common air, without any sensible influence of the sun's heat, and yet nothing properly volatile is to be found in iron, or its vitriol? Such things put the minds of men to a sort of nonplus. Yet still we must take them as we find them, and be determined by nature, and not by our reasonings.

I resolved next to have a greater quantity of our water, for evaporation to a residuum, and to prepare it better for dissolution of its compounding parts by putrefaction. For this purpose, I put twelve English quarts into a small cask, which I shut up close, with the mouth pitched round, and placed it in a cellar during five weeks; and put about six quarts more in

glass-bottles, some simply corked, others also pitched, others open, some exposed to the morning sun, others in a cellar. I told before, that, by my smell, taste, or trials, I could find very little alteration in our water, other than the taste and smell the simple element has in such case. These eighteen quarts I put on the fire to boil and evaporate in two brass pans, one smaller, the other larger; the smaller was open for most part during the boiling, the larger was covered from beginning to end. The quantities in each I did not mark. I had of sediment of the lesser, gr. xxxiv. of a grey dusky powder, as in evaporations formerly. The residue of the larger I observed coming to the consistence of bitumen, and of a black colour, the decoction accordingly smelling and tasting high of the same kind. I poured off a little remaining water of the decoction, and, dropping on it some guts of *ol. tart.* I found still the cloud formed, and standing all the day without any curdling precipitate. I found the bitumen, after the water was evaporated before a fire in a tea-plate, weighed gr. clxx; and, exposing it again to the fire till it became a rosin, I had drach. ii. gr. xi. Of this I threw several parcels into a red-hot crucible, placed in an ordinary fire of no intense heat, and every parcel quickly flamed about two minutes, then almost vanished. I next tried the powder I had as residuum of the smaller decoction; but it did not flame, but jetted out sparkles of flame, huzzing as wet gun-powder does, when touched with fire. I can attribute this variety to nothing else but the

the larger pan's being covered during the decoction, whence I had the bitumen; and, upon sight of it, compared with its taste and smell, concluded that I should have my residuum more inflammable, or easier to take flame than formerly. Of such consequence it may be, more than we ordinarily think on, to have our decoctions, whatever they be, more or less circulated.

Upon the whole, it may appear, that analogy right stated, and justly regarded, is the surest way by which our reason is capable to lead us into the nature of things; and that experiment, justly stated and managed, will always answer to such analogy. And hereby is confirmed the likeness of our water to that of Scarborough, as to their specific contents, though in proportions different from each other; the salts abounding more in that of Scarborough, the sulphur appearing to abound more in ours, the different bulk or weight of the whole considered, which is much greater in Scarborough water than in ours. And yet it will next appear, that equal quantities drunk, the physical operation of ours hath proved not inferior. Analogy hath taught me the presence of these salts; Dr Short and Dr Shaw have found them by direct trial. Thus does the fact confirm the analogy, as I stated it betwixt the experiments on different substances, and these on our well, and of consequence betwixt our well and that of Scarborough. The evidence appears equally concerning the nitre and sea-salt in both. Concerning the respective quantities whereof in each water, I had occasion

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to make an observation pretty remarkable, but whereon at first I was little thinking.

Having remarked, that a parcel of the Scarborough residuum, two or three days after evaporating its moisture, appeared considerably more bulky, I weighed and found it drach. ii. very near, then put it in a tea-plate before a warm fire two hours, and found the weight diminish near one third, which answered to my suspicion, that its comparative bulk was from the moist air it had attracted, the weather having been somewhat foggy. I had observed no such appearance in any residuum I had got of our well: But, to be more sure, I tried in the very same manner, while the weather was moist, gr. xviii. I had remaining of the residuum of last decoction. When the plate was taken hot from the fire, I found the weight diminished not above gr. i. sem. This with the comparative colours I always observed of the residua of each, that of Scarborough water going constantly on the grey white, and that of our well on the dusky brown, less or more intense, proves still the prevalence of the salts in Scarborough water, if not of the calcareous earth, and of the sulphur in ours. To which, finally, may be added, that, having brought my last decoction of our water to about a gill, I dropped thereon some *ol. tart.* and it formed a thick cloud of an equable surface and consistence all over, and standing firm several days, without separating till broken on design, which had not occurred to me before in any former trials; and it appears above,

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bove, that decoction of Scarborough to one half, gave no such appearance.

P. S. As I could not, by evaporation, bring our water at any time to cast out any crystals, I designed, at last decoction, to have tried the salts adhering to the sides of the pan; but, not finding how to determine any thing thereby, and fearing to transgress my bounds, I caused them to be washed down into the decoction.

IX. *Medical Qualities of Montrose Well, with some instances thereof; by the same.*

THE water of this well is of an obscure mineral taste, which cannot well be described. It is lighter on the stomach, and easier to digest than the finest ordinary spring water, or any other of our mineral springs; so that delicate constitutions bear it easily, and are improved by it in their appetite and digestion. A man about seventy years of age, and of a decrepit habit, bore easily, even when he began first to drink it, two English quarts in the morning, and near as much after noon, with alleviation of a cough he laboured under, and told me, though he was of a low station of life, he could never formerly bear a draught of any manner of water.

It clears the spirits of heaviness, and gives life to the inward powers, even of these of the more sower and low diet. The simple element, whether cold or hot, infused on tea, by diluting

ting, and in either way, by its stimulus, washing off the indigestion, which clogs the natural faculties, in those of high or full diet, serves so far well to this purpose. But those of slender constitutions, and who live on low diet, have found this effect by a few cups of this water. Some of the more sober and delicate females have also experienced this. And a gentleman using the vegetable diet, and of the animal nothing above milk or eggs, and the simple element for drink, of a thin habit of body, and with lively spirits, found himself generally yet more chearful, and his appetite and digestion improved, by drinking only about an English pint.

It is universally diuretic, people accounting generally that they pass more that way than they drink.

Being drank from two to three pints English, it generally purgeth by stool three or four times, the half of that quantity succeeding in some. By this quantity a gentleman, from beginning to drink of it, was set a purging three or four days together, during which he was obliged to refrain its use, and afterwards to diminish the quantity, till he was more accustomed to it. The more plentifully one feeds, it generally operates more, as hath been observed concerning other purgative spaws. In some, the same degree of operation hath continued during the use of the water: But generally, as people are more accustomed to it, they are obliged to intermit at times its use, or to augment the quantity.

Soon after its medical virtues were first observed, it performed a cure, scarce inferior, I

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believe, to most, or any recorded in observations of medicine. A girl during nine years, almost from a child, afflicted with strangury, stoppage of urine, and, for most part, of the evacuations of her belly also; her urine being constantly as a thick gleet, and resembling the mucilage of oat-meal; she having, during most of the above time, scarce any intermission of symptoms, whereby her flesh became wasted, and her strength so much impaired, that she went on crutches, or otherwise supported. At length the paroxysms taking her constantly so soon as asleep, she became as one insensible, talking, and answering things spoken to her out of purpose. She being then supported to the well, began immediately, being under no direction, to drink freely, and it passed by stool and urine as freely, with immediate ease. This encouraged her to too much freedom, till, over-charging nature, she was threatned with a suffocation, and hereby was taught to use it more moderately. I gave her some directions for drinking it afterwards, and, the evacuations still succeeding, she not only was cured of all disease in a few weeks, but also became of a clean and natural full habit of body; when she declared, that, for nine years bygone, she had not enjoyed so much freedom from trouble, taking all in one, as she had since she began to drink the water.

In general, as a good many, in various symptoms of gravel and nephritic pains, have come to it for relief, so I have found none,
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who have not been more or less, and most of them remarkably, relieved by it.

A gentleman, during a good many years, afflicted with paroxysms of the stone, rarely under twice a year, and sometimes they seized him more frequently, each paroxysm only going off after passing stones of various bigness, by drinking in the morning the above quantity, or a little more of this water, it going off plentifully by urine, and by moving the belly a little, became fresher in all his natural and animal faculties, and has continued almost a year free of all nephritic paroxysms, except some small threatnings. He then left this place, and I know not how he has been since. One circumstance in his case was most remarkable, that, during the use of the water, he passed urine always without any stimulus or inclination to pass any more, which had constantly affected him before, since the first attack of the disease.

Another gentleman, during several years under grievous symptoms of strangury, and sometimes passing gravel, several means of relief, and particularly the Aberbrothock water, being used to no purpose; at last there was strong suspicion of an ulcer in the neck of the bladder where he ailed most, from a purulent matter passed with his urine. After all hopes of life were almost gone, he began insensibly to become easier of the symptoms; but the disease still remained, and particularly his urine passed with difficulty, and he had violent pain from the strangury always after passing it. From beginning to drink a pint

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or little more of the Montrose water in a morning, and drinking it warm infused on tea after noon, his belly being opened a little after his morning draught, he began to pass his urine every day with more freedom: The pain diminished also daily; which good effects he had not found before from any water, simple or medical, neither from any other liquor he drank.

As it generally relieves in all scorbutic symptoms, and foulness of the blood, so hath it perfectly cured several inveterate scurvies in the younger and older, above any effects of medical waters I have hitherto observed.

A gentleman, otherwise of a strong constitution of bowels, and of a vigorous body, but labouring a good many years under the foulest eruptions on his skin, almost coming to a degree of leprosy, by drinking from three to four English quarts a-day, which gave him three or four stools, and dipping his shirt among the water for a bath over night, was, from the worst degree of eruption he had been liable to, brought almost incredibly to a perfect cure, in less than three weeks. The eruptions that were thick, inflammatory, and sending out a fumes, as I observed them in the beginning, going all away, and his skin becoming all over soft and smooth, as I also saw; and he has continued sound and in perfect health now above a year, sometimes drinking the water at about six miles distance.

A woman of about sixty years of age, of a valetudinary state of health, and particularly much afflicted, during several years, with

scorbutic runnings over several parts of her body, her head also being greatly affected in the same way; diet-drinks of several sorts given and repeated proving of no use, by about a month's use of about three English pints of this water a-day, whereby she purged three or four times daily, was entirely cured, and remains so now more than a year.

A girl seven or eight years of age, labouring, almost from her birth, under a dry itching scurf over her skin, and several measures being taken for her cure in vain, by drinking three weeks daily, about two thirds of a pint of water a-day, whereby she had the ordinary stools, and bathing therein twice a-week, was made entirely clean. And the disease having threatened sometimes to return by unguarded treatment, she hath been always cured by the same means, and is now perfectly sound.

By other instances of cures made, or relief given by the use of our water, it would seem to be of pretty good service in most chronical diseases, and to reach from the first organs of digestion, to all that affect the blood. In winds and flatulency stifled in the bowels, it hath proved of good use, breaking the wind with such force, that people, the females especially, have been afraid to drink it in public; and some who have been long afflicted this way have been perfectly cured by it. Others have found benefit by this water, who could not bear drinking the simple element cold. So hath it also made compleat cures by stool and urine in spasmodic colics, called by the vulgar

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vulgar windy gravel. To this class we may even likewise bring the case of a woman labouring under racking pains of her breast and head, who, having passed turbid urine in large quantity, by drinking it, was cured without any purging.

It has also been beneficial in disordered secretions from the blood, and where the liquors have been in a stagnating way. Among the rest, the case of a young man was remarkable, who, after a tertian ague of three months continuance, having swelled all over the belly, arms, and head, was cured, by drinking, without allowance, what he could of the water, from sun-rising in the beginning of August, to about nine o'clock every day of two weeks. But the case following is more singular.

A woman falling into blood spitting, (supposed to have been occasioned by a strain some time before), which continued about a year and a half, recurring less or more every three, or at most four days, and frequently sooner, notwithstanding her menstruating regularly, from the time she began to drink this water, which she continued for two weeks or more, she spit none, neither did any bad symptom appear, although the water operated every day, both by vomiting and stool. She began by drinking two quarts. On hearing which, to prevent such operation, I ordered her to take not above the half; notwithstanding this, it continued to have the same effect; and I am just now told, that she continueth well.

In rheums and strains also I have found it of good use, by way of cold pump, where the persons could not bear such use of ordinary cold water.

A gentlewoman of a thin habit, and advanced age, being afflicted with a rheum of a pretty long continuance in her arm, had the pain allayed, and the rheum as good as quite cured, by the water used in that way: But the rheum some time after attacking the other arm, she was afraid to admit of the cure.

A gentlewoman having strained her foot, whereby, notwithstanding variety of cures, she was confined during most of a year, could not move her toes, nor put her sole flat and even to the ground, the flesh of her leg being also remarkably wasted, her skin became discoloured, rough, and as it were dried; not having been able to bear the application of ordinary cold water to the recent strain, by bathing and pumping this water on her foot and leg, about two weeks, she began to move her toes, then all her foot freely, and rested it level on the ground; her leg also recovering its native colour and softness, and becoming plumper; so that she began to walk about a little, supported by a crutch, and sometimes only by a staff, when, by an unhappy accident, falling on the same leg again, and her foot folding as it were under her, all her ills returned to almost a worse condition than in the beginning: And she, after not finding the immediate benefit of the water, was impatient to lay it aside, that she might try other cures, which hitherto have had no success.

A poor man, at once paralytic of his limbs, and wrong in his judgment for several years, as I am well informed, coming to our well on crutches, without which he could not stir; in a few weeks, by drinking what he could get of the water, and bathing his limbs in it when he found none to hinder him, was so well cured, notwithstanding his being seen drunk at times, that I saw him walking about sound, carrying his crutches.

I could have given other instances of cures done by this water, had not my avocation in the country, and business when in town, detained me from so closely attending the well, in this, as in the former season. But, perhaps, these are enough for the bounds of this paper.

The disadvantages of our well are, its being a little too much exposed, being close on an open road, without convenient covering for walking about: This could not well be helped, without some apartment being erected for the better sort, females especially. Then the spring is but small, giving only two English quarts in a minute, too little for affording sufficient water for the demands of bathing the body, but more especially for the drinking of any confluence of people. To remove both defects, I will endeavour, next spring, to have a stone-bason, with a cover, made, as a reservoir for the water in the ground below the well.

X. *An Essay on the Method of preparing and preserving the Parts of animal Bodies for Anatomical Uses*; by ALEXANDER MONRO, Professor of Anatomy in the University of Edinburgh, and F. R. S.

THE most artful management of the knife alone cannot discover the texture of animals sufficiently; anatomists are therefore obliged to employ several other arts, which often require a considerable time in their execution, and frequently fail through some unfitness in the subject, or by a very small omission or accident: And, when they succeed in any attempt of this kind, they endeavour to preserve the parts thus prepared, that they may always be in readiness to supply what cannot be demonstrated on the recent subject. While each anatomist is obliged to his own industry only for the discovery of these arts of preparing and preserving, it is probable that frequent disappointments, and the despondency these will create, may discourage many from pursuing the study of anatomy, who might become the greatest improvers in it, if these difficulties did not lie in their way. With a view to remove them in some measure, I shall lay before you in this paper such arts of preparing and preserving the parts of animal bodies as I have found successful, which may serve as a foundation on which others more industrious or ingenious may build.

The principal preparation that BONES require is to make them white; for which directions

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rections are given at considerable length by Paulli * and Lyferus †, who also teaches the method of putting the bones of adults together. What I have found most successful for whitening the bones of young creatures, is macerating them long in cold water, which ought to be changed frequently; the bones being laid out each time to dry a little in the sun. If they lie too long in the water, even those of adults will dissolve in their more spongy parts, and the younger ones lose all their epiphyses. If they are allowed to dry before the blood in their vessels is melted down by the maceration, it scarce can afterwards be separated from them, or they will never become white. The marrow of young bones being much less oily than in adults, they generally can be made much whiter, and do not readily turn yellow by keeping. The bones of foetuses require to be taken frequently out of the water; and the periosteum must not be separated where the epiphyses are joined, otherwise it is scarce possible to prevent the loss of these additional pieces.

The method of burning and exposing the bones of adults long to the weather, for unravelling their texture, is so generally known, I need scarce mention it.

CARTILAGES are made pellucid in the same way that bones are whitened, and they must afterwards be brought to their natural shape and situation, in which they are to be kept by strings,

* Act. Hafn. vol. 2. § 18.

† Cult anat, lib 5.

strings, weights, pins, and such other contrivances, if they are to be preserved dry.

The MUSCLES that have no large cavity are to be laid and secured in the posture they are designed to be preserved in, and they are pressed with the fingers into a natural shape while they are drying.

You have already published all I know of the method of injecting the VESSELS. [See Art. IX. of vol. I.] That they may appear better, it is necessary to macerate in cold water all injected parts that are coloured with blood, till the blood is extracted; after which the water is to be pressed well out; and, even when the preparation is to be preserved wet, it is of service to let it dry a little in the air, before it is put into the embalming liquor. But, before the very minute extremities of injected vessels can be demonstrated, there is a farther art requisite, which is the same that has been employed by several of late for unravelling the texture of leaves and fruits, of which Severinus *, near a century ago, says, “Verum
“me ineptum, quid præterieram opontii folii resolutionem artificiosam : Parabitur hæc porro
“simplici tabefactu per affusam uberem aquam
“tamdiu complexuram, dum fibris lignea duritie
“restibilibus, omnis exsolvatur carnea portio.” And Ruysch † at last acknowledged his method of preparing the succous vessels of fruits, and of the brain, &c. to be the same. Put therefore the injected brain, lungs, liver, spleen,

* Th. Bartholin epist. med. cent. 1. epist. 65.

† Advers. anat. dec. 3. § 2.

spleen, placenta, or any other part of a tender texture into water, allow it to remain there till its involving membrane is raised by the water insinuated into the cellular membrane, which connects it to the parts below; then separate the membrane, and afterwards keep the part among the water, till the fibres, connecting the small vessels, are dissolved: This is known by shaking frequently the part among the water, which washes off the corrupted particles, and at last the succous vessels are seen distinct and floating in the water; then the preparation is taken out, and, by gently pressing, the water is squeezed from it, the remainder of it being washed away with some of the preserving liquor, into which it is immediately put, and, by a little twirle of the thread or hair by which it is suspended, the preparation is expanded, and the small vessels are separated.

I never could divide nerves into their very small filaments, after they had got their firm coat from the *dura mater*; but, before they are involved in that membrane, they are easily divided. Those that constitute the *cauda equina* are fittest for this purpose; for they are long, and their fibres adhere by very weak fine membranes. One of these chords being cut through, where it comes off from the *medulla spinalis*, and where it is about to enter the *dura mater*, one end is secured with a hair, by which it is suspended in a basin of water; and, after macerating some time there, it is raised to the side of the basin, upon which it is laid, while one hand holding the hair, with the

the other a very small needle fixed in a handle, is slightly striped along the nerve. Let this operation be continued, till, upon twirling the nerve among the water, it is expanded into a fine web of very small fibres, when it is put into the embalming liquor. If the blood-vessels were previously injected, the hair must be tied on the end of the nerve nearest to the *dura mater*, that the trunk of the nerve and artery may appear together: When a piece of the *cauda equina* is thus prepared, it shews very prettily; for each filament almost of the nerve appears with injected vessels upon it.

When a fine fine MEMBRANE, such as the pleura or peritoneum, is to be preserved for demonstrating its arteries after an injection, as much of the cellular membrane, by which it is connected to the neighbouring parts, is to be saved in dissecting it off as can be done without spoiling the transparency of the membrane; for, when the cellular substance is wholly separated, very few continued ramifications of vessels can be seen, a great number of exceeding short extremities only appearing, which require a microscope to view them distinctly. When there is little fat lodged in the cells, the membranes of the cellular substance may be left without a possibility of observing them: When they are filled with fat, it must be pressed out as much as possible, after being well macerated. Even when membranes are to be preserved in a liquor, they are more easily kept extended in it, and their vessels are better seen, if they are previously dried. In doing

ing this, they must be extended with pins or threads on a finely polished clean board, or it is rather better to keep them on the stretch while they are raised up from it, that they may bear no print of the board. After membranes are dried, their doubled edges, or other inequalities, are to be cut off with a pair of scissors.

Ruyfch * describes the manner of separating the cuticula and *corpus reticulare* from the skin, by stretching these common teguments, well freed from the fat, on a board, with the cuticula outmost; and then dipping all into boiling water, which loosens the cuticula and *corpus reticulare* so from the skin, that they are easily separated with a blunt knife, or the thin ivory handle of a scalpel; then with the same instrument he separates the *corpus reticulare* from the cuticula, leaving them connected to each other, and to the skin in some part. After this they may be either dried or put into the embalming liquor. When either the *corpus reticulare* is not made firm enough by the hot water, or happens to be very thin, it is difficult to separate any considerable piece of it entire from the cuticula.

A chirotheca or podotheca, a glove or a shoe of the scarf-skin, with the nails adhering to it, is brought off with very little trouble after the cuticula loosens from the parts below by the putrefaction, which keeping a subject long brings on. This method answers the design better than forcing off the scarf-skin with boiling

* Adv. anat. Dec. 3. § 2.

boiling water, which makes the cuticula tender.

The cellular membrane under the skin cannot be preserved distended with air, except where there is little or no fat contained in it. One of the best parts for making a preparation of this kind is the scrotum, where what is commonly called *musculus dartos* may, by blowing into it, be changed entirely into fine membranous cellules; and Carolus Stephanus * very well observed, that the cellular substance any where else under the skin puts on a muscular appearance when the fat is wasted. Will not these observations serve as some apology for former anatomists who reckoned a *tunica carnosa* among the common teguments of the body? May we not hence see one reason of old emaciated people having so many wrinkles in their skin?

That the DURA MATER with all its processes may be preserved in a natural situation, it is necessary to saw the cranium from near the root of the nose to the middle of the *os occipitis*, by a perpendicular section at half an inch distance from the sagittal suture, and then by an horizontal section, terminating at the extremities of the former perpendicular section, to take off a considerable share of the side of the cranium. After which the *dura mater* being cut by an incision in form of a T, the brain and cerebellum are taken out, and the head is put amongst a liquor to preserve it; or the bones are made clean and

* De dissect. part. corp. human. lib. 2. cap. 2.

and exposed to the air to dry, taking care to keep the cut parts of the *dura mater* stretched out with pins, hooks, or threads. If the head of a foetus, or of a very young child, is thus to be dried, the ligamentous membranes between the bones must be kept extended by sticks cut of such a length, as, when put into the cavity of the cranium, their extremities may rest on the bones, and push them outwards.

The processes of the *PIA MATER* which are placed in the interstices of the convolutions of the brain, may be easily separated entire with the *pia mater*, when that membrane is preternaturally thickened by diseases, as it frequently is: And, even in a sound state, large pieces of it may be got with its processes, after macerating the brain in water. As soon as it is separated, and the water pressed from it, it ought to be immersed in the embalming liquor, where it is to be kept extended by threads or small branches of plants.

The *BRAIN* requires no preparation, except either for demonstrating its succous vessels, the method of doing which I have already mentioned; or for hardening it, which I shall afterwards speak of.

Before the coats, humours, and vessels of the *EYE* can be rightly prepared for preserving, so as to demonstrate all of them, it is necessary to coagulate the crystalline and vitreous humours, by immersing the eye for some time in a proper liquor; of which hereafter. After this, they will bear maceration in water,

for the separation of the choroid and Ruysch's coat.

The sebaceous glands and ducts of the eyelids appear much better after a subtile injection of the arteries, and the coagulation of their liquors, than in the recent body.

Dr Trew * has very justly observed, that, by macerating the EAR in water, the membrane which is continued from the epidermis of the ear to line the *meatus auditorius externus*, and to form the outer lamella of the *membrana tympani*, may be brought off entire in adults, as well as it is easily separated in fœtuses, or very young children. And indeed the *membrana tympani* appears to be no other than this cuticula, and the membrane that lines the tympanum, connected by a thin cellular substance, in which, as in all other such parts of the body, the larger branches of the vessels run.

The epithelia of the LIPS, as Ruysch calls the cuticula covering the papillæ there, is to be taken off by macerating in water, which makes the surface appear better in the villous way, when the lips are afterwards put into a glass with the embalming liquor.

The villous substance of the TONGUE is very easily made quite red, with injection thrown in by the arteries, and a membrane analogous to a cuticula separates by soaking in water; and, upon comparing the lips, tongue, œsophagus, stomach, and intestines, the structure seems to be so far alike in all, as they are covered with this sort of cuticula, which is connected

* Act. physico-medic. Acad, N. C. Vol. 2. obs. 56.

connected to the muscular part by a cellular substance, in which the numerous nerves, vessels, and glands are lodged. This cellular substance either is formed into ridges and valves where it is thick and loose, or appears like a fine membrane where it is thin and stretched.

There are no organs in the body of which I find a greater difficulty to give the students of anatomy a good idea, than those employed in deglutition. In the recent body, they cannot see them all at once in the natural situation: They can scarce be held in a wet preparation, so as to shew them well enough. What has the best effect, is to demonstrate the grosser parts first in a dry preparation, which requires patience to execute right; for all the muscles belonging to these organs, that are fixed to any of the surrounding parts, must be all clean dissected, and then cut off from these surrounding parts; after which the tongue, *os hyoides*, *fauces*, *velum pendulum palati*, with the uvula, the larynx, pharynx, *aspera arteria*, and œsophagus, are taken out with the dissected muscles hanging at them, to be all secured in their natural situation with small pieces of thin boards, and threads fastened to hooks. A cork is then put into the lower part of the trachea, round which and the œsophagus a strong thread is tied firmly, and quick-silver is poured by the fauces, or by the passage which formerly led to the nostrils, till the œsophagus, trachea, larynx, and pharynx, are filled; in which condition it is allowed to hang till the parts are pretty firm, but not near fully dry, when the quick-silver is poured out, and

the parts overstretched by its weight, such as the glottis and space between the tongue and larynx, are, by pressing and squeezing with the fingers, to be brought near to the natural situation; and others that shrivel too much, as, for instance, the uvula and epiglottis, are kept near to the natural form, by drawing and pressing them from time to time till they are fully dried.

The hollow VISCERA of the thorax and abdomen are not only to have their vessels demonstrated in the ways mentioned formerly, when speaking of the blood-vessels; but, when they are to be kept dry, they require a particular preparation for preserving their form, and for shewing their interior structure, which is to fill them with some proper substance. The properties I think this should have are, to be able to resist the shrivelling contraction of their fibres, to fill them all equally, and to leave them clean if it should be taken out. For these reasons, cotton, wool, sand, or such like, are improper; all that I use is air, quick-silver, or melted wax.

Wax is only to be employed where there is no farther design than to view the exterior surface; for which purpose it may be thrown into any of the hollow bowels; but, in all other cases, air or quick-silver must be used.

Where air will answer the design, it is preferable to the quick-silver, for it stretches every where equally; whereas mercury presses most on the depending parts. Air dries the bowels in the twentieth part of the time that quick-silver does, and it leaves no colouring or gilding

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gilding on them; which the mercury always does. On the other hand, air does not stretch some parts sufficiently, cannot be retained, and insensibly escapes so from others, as to allow them to collapse in drying; which inconveniencies the quick-silver is not so subject to.

From what has been said, it is evident, that air is required, or is much preferable to quick-silver, for making dry preparations of the œsophagus, stomach, guts, *vesica fellea*, with the biliary ducts, and bladder of urine, with the ureters. And it is as plain, on the other side, that the pericardium and uterus can only be kept distended in their natural form with mercury.—The heart with its blood-vessels, and the pelvis of the kidney with the ureter, generally have some small passages for the air to escape at, and it scarce can resist the shrivelling contraction of their fibres; wherefore mercury is preferable in drying them. The *corpora cavernosa* of the penis, and the *vesiculæ seminales*, retain both air and mercury; but this last leaves a gilding in the *corpora cavernosa*, which hinders such a clear view, as could be wished, of their vessels and texture; and there is a difficulty to fill the *vesiculæ seminales* with it, for the orifices at the *caput gallinaginis* will not admit it from the urethra; and, when it is poured in at the *vas deferens*, the moisture of this narrow pipe is liable to stop it; and, after you have got it to run in this canal, its weight forces open the orifice into the urethra of the small duct common to the *vas deferens* and *vesicula seminalis*, so that it will not mount into the vesicula till the urethra is filled: Where-

as the contraction of that orifice resists a small blast of air, which readily regurgitates into the vesicula. For which reasons, air is preferable for the *corpora cavernosa* and *vesicula seminales*.—It is seldom we meet with subjects whose lungs or spleen will retain air, and the glans of the penis is very liable to allow it to escape; wherefore, generally, we are obliged to make use of quick-silver for these parts; which, however, does considerable prejudice in all of them, but especially in the lungs and glans, whose cellules are smaller than those of the spleen.

Being determined, by the foregoing rules, which of the two liquors to use, we press out all the blood or other liquors contained in the bowels we design to fill, and then tie all the passages from them, except the one by which we are to introduce the distending liquor; and, in case we discover any others in the time of filling them, at which the air or quick silver escapes, we tie it likewise.

The passage by which the liquors should be introduced, is to be chosen such as soonest allows them to pass every where into the cavity to be filled, and that is most easily secured afterwards. What was said of the organs of deglutition, will readily lead one to know how to distend the pericardium and uterus. The place for blowing up the alimentary tube, *vesica fellea*, and *urinaria*, is known to every body. Liquors are poured into the heart and large arteries through the superior *vena cava*, and any branch of the pulmonary veins. The *trachea arteria* receives what the lungs are

to be distended with. The kidney is to be filled by the ureter. The veins of the spleen, *capsula atrabilaris*, and *corpora cavernosa penis*, are to convey the substance with which they are distended.

We must always make use of a pipe, when we are to blow up any part. The best sort of pipe for this use is what has a small extremity, with a notch round it, and a stop-cock a little higher in it. We introduce the small extremity into the proper canal which is tied upon it with a waxed thread, that is made to sink into the notch; and, as soon as the bowel is sufficiently distended, the stop-cock is turned, to prevent the air to get out. If any of it escapes, it is easily supplied by a new blast at the pipe, which is supported by some string or board to hinder it to press or draw the preparation while it is drying.——If the anatomist is provided with no other than a common blow-pipe, the canal by which the air is introduced must be tied tight upon it with a thread, which is drawn with a firm knot by an assistant, while the air is blown in. As soon as the bowel is sufficiently distended, a signal is given to the assistant for pulling the ends of the thread, while the pipe is drawn out of the canal, and immediately another knot being made, the preparation is suspended by this thread.

When quick-silver is employed, the passage by which it is poured must be higher than any other part of the preparation; and when that passage is narrow, a small glass funnel or pipe must be put into it. This pipe must be long, where

where the weight of a high column of mercury is wanted to make it run through small vessels. If the preparation will allow, the passage by which the mercury entered is to be tied firmly, otherwise, before any mercury is poured in, that passage must be secured, so as to remain uppermost all the time the preparation is drying.—When a considerable quantity of mercury is poured into any part whose texture is tender, it is necessary not only to suspend it with threads and hooks in the superior part, but to support it also by a small net extended below it, upon a proper vessel for receiving the quick-silver, if by any accident it should run out.

The directions now given will serve for preparing the most part of the bowels; but the **LUNGS** and **SPLEEN**, whose membranes very difficultly retain either quick-silver or air, especially this last, require more care. These bowels must not be taken indifferently from any subject; but such must be chosen as have the exterior membrane thick and strong. After they have been distended as above directed, they should be exposed to the warm sun, or near a fire to dry them soon, returning frequently to supply by a new blast what they lose in very little time. As soon as their outer surface is dry, immerse them into strong turpentine varnish, so as their whole surface may be covered, after which they will retain the air much better; continue to place them so that they may soonest dry, taking care to rub on varnish with a feather, where-ever it is wanting,

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ing, and to blow in new air whenever they subside.

After the human spleen has been kept distended with quick-silver or air till it is dried, it seems to be wholly composed of cells communicating with each other, upon the sides of which the small branches of the artery are observed to spread in great numbers, if they have been previously injected, as you may see when you please in several such preparations now in my possession.

Upon cutting the lungs thus prepared, their vesicles appear far from being spheres, of any other figure whose transverse section is circular, for they evidently are polygons, generally irregular squares and pentagons. And indeed one might conclude *a priori*, that they are rather more so in a living creature; for, seeing the exterior membrane is of a firmer texture than the vesicles, and will not stretch out so far as they could be extended, they must push violently on each other; and therefore be pressed from a spherical form into as many sides and angles as there are contiguous vesicles; and the thorax of a living creature does not allow the lungs to be extended so far as their exterior membrane can be stretched, as is evident from their starting out at wounds of the thorax, or by blowing into the *trachea arteria*, after the sternum of a creature is taken away; and therefore their vesicles must be more compressed, and consequently their sides be straighter in respiration, than when they are distended after being taken out of the body. These considerations, and the obvious

obvious polygon form of the cells in the simple lungs of serpents, frogs, &c. makes me surprised that ever the vesicles of more complicated lungs should have been imagined to be spheres, or any other figure, whose transverse section is circular.

I come now to consider the manner of preserving preparations, which is either by exposing them to the air, till all their moisture is evaporated, and they become dry, rigid, and out of hazard of corrupting; or by immersing them in a proper liquor. Besides what has been already said concerning the manner of drying preparations, it is also necessary, especially when the parts are thick and bulky, and the weather is warm, to guard further against putrefaction, to hinder flies to lay their eggs upon them, which soon growing into maggots would destroy them, and to prevent insects, mice, or rats, to feed on them. All this may be done by soaking the preparation, some time before it is exposed to dry, in a solution of corrosive mercury in spirit of wine. The proportions I use, are two drachms of the corrosive to a pound of the spirit; and while it is drying, it is to be moistened frequently with the same liquor. By this method one can, without any of the dangers above mentioned, dry the dissected bodies of pretty large children in the midst of summer, which sooner answers the design, than the cold moisture of winter can.——After the preparation is dry, it is still liable to moulder away, become brittle, crack, and have an unequal surface;

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face; wherefore it is necessary to defend all its surface with a thick varnish, with which it is to be covered over as often as the lustre of the former application wears off; and it must always be kept well from all dust and moisture. The varnish I always use is the common spirit one, which I buy from the painters *.

Dry preparations are very useful in several cases; but there are a great many where it is necessary the parts should be flexible, and nearer a natural state, than the shriveling and rigidity in this way of preparing can allow: The difficulty has hitherto been to find a liquor that would preserve them so near to a natural condition. Watery liquors do not prevent the putrefaction, and dissolve the hardest parts of the body. Acids prevent putrefaction, but dissolve the parts into a mucilage. Ardent spirits harden the parts, change their colour, and destroy the red colour of the injected vessels. Oil of turpentine, besides the faults of the ardent spirits, becomes thick and viscous. But, without dwelling longer on the faulty liquors, I shall inform you, That what I have always found to answer best, is a rectified ardent spirit, no matter whether from wine or malt, that is still limpid, without having any yellow tincture, to which a small quantity of a fossil acid spirit, such as that of vitriol, or nitre, is added; both of them resist putrefaction, and what might be looked on as faults, if each was considered single, is mended by the other.

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* Ruysch recommends a varnish made of *gum. copai.* and *ol. spica.*

When these are mixed in a right proportion, the compound liquor changes neither the colour nor consistency of the parts, except where there are serous or mucous liquors, which it hardens much in the way that boiling water does. The brain even of a new born child is made so firm by it, as it can be handled with great freedom. The crystalline and vitreous humours of the eye become firm, but white and opaque, by soaking in it. The liquor of sebaceous glands, of mucous folliculi, the seed, &c. are coagulated by it. The lymphatic, or watery liquors, such as the aqueous humour of the eye, water of the pericardium and amnios, are not changed by it. It heightens the red colour of injected liquors so much, that vessels, which did not at first appear, are plainly seen, after the part has been in it some time. If you'll compare these effects with what Ruysch has said here and there of his preparations, you'll find that the liquor described comes pretty much up to the properties of his balsam, as he calls the liquor in which he preserved his wet preparations *.

The proportion of the acid to be mixed with the ardent spirit, is to be varied according to the part to be immersed in it, and the design the anatomist has. When the brain, humours of the eye, &c. are to be coagulated, a larger proportion

* Ruysch's balsam, according to Risterus, (*Notit. rerum natural. et artific. et*) is rectified spirit of malt, *frumenti*, to which a third part of water is added. Ruysch distilled this spirit in a tin alembic, with a slow fire; and thought that the addition of a handful of black pepper made the spirit penetrate better into the fleshy parts.

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portion of acid is necessary, such is, two drachms of spirit of nitre to a pound of alcohol. When the parts are only to be preserved, especially if there are any bones in the preparation, forty, thirty, or fewer drops of the acid will be sufficient. If too large a proportion of the acid is mixed, the bones become first flexible, and then dissolve.

After any part is embalmed, particular care must be taken to keep it always covered with the liquor, otherwise its colour spoils, and some parts harden, others dissolve. To prevent therefore the evaporation as much as possible, and to exclude the air, which makes the spirits extract a tincture, the mouth of the glass is to be carefully stopped with a glass or a waxed cork, cover, or stopper; over which leaf-tin, a bladder, or injected membrane, is tied, in which way it will keep a considerable time without any great waste. Whenever the spirits sink near to the top of the preparation, more of the spirit of wine must be added, without the acid, which flies little off. If at any time the spirits acquire too high a tincture, they must be poured off, and new spirits, with a less proportion of the acid than at first, must be put in their place, the old ones being kept in a well corked bottle, for washing away the natural liquors of any new preparation, which ought always to be carefully done before any part is embalmed, and the old tinctured spirits are likewise to be washed off, with a small quantity of limpid spirits, every time the old ones are renewed, or a new preparation is to be preserved. The spirits unfit to be longer shewn in glasses,

may also be still employed in preserving parts in earthen vessels or glass ones, out of which the preparation must be taken when it is to be viewed.

It may not be amiss to remark here, that the glasses in which preparations are to be demonstrated, ought to be of the finest most transparent thick kind, for these allow the parts to be seen most distinctly, without changing their colour, and at the same time magnify the objects, discovering parts which, when they are out of the glass, could not be seen with the naked eye. Since then the glass with the liquor has a certain focus, where objects are seen most distinctly, it will be necessary to use some contrivance for keeping the preparation at the proper distance from the side of the glass; which may be done by putting in a small branch of some plant, or a little stick, or by fastening the thread or hair by which the preparation is suspended towards one side of the glass. Such little art, for keeping the parts expanded, or placing them so as to be seen to the best advantage, will easily occur to any who practises anatomy.

I shall finish this essay with a caution to anatomists to shun as much as possible putting their fingers into this acidulated spirits, or handling the preparations that are very wet with them; for they bring such a numbness on the skin for some time, that makes the fingers unfit for any nice dissection. The best cure I have used for this numbness, is washing the hands with water, into which a few guts of *el. tartar. per deliquium* have been dropped.

XI. *Some*

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XI. *Some Thoughts concerning the Production of animal Heat, and the Divarications of the vascular System, being an Abstract from a Latin Treatise of the Heat of Animals: In a Letter to Dr JOHN STEVENSON Physician in Edinburgh, from Dr GEORGE MARTIN Physician in St Andrew's.*

S I R,

I Cannot but acknowledge the pleasure I had from the good opinion you conceived of my treatise, *de Calore Animalium*. The objections you was pleased to propose against it, gave me full proof you had condescended to read and consider it carefully; and at the same time gave me an opportunity, if not to obviate them entirely, at least to render the scheme somewhat less exceptionable. You think it would be of use to the world, and wish it were made public: But I do not yet think it finished; and I know not if any works of mine shall ever arrive at that degree of perfection, as to be able to bear the light, and face the public by themselves. In the mean time, in compliance with your desire, which I shall always very much regard, I will make a short extract of some of the most material things of it, which, after revising, you may dispose of in any way you think proper.

I need not say much concerning the universality of this property of heat, whereof all animals, while in life, have a considerable share. Nor is it necessary to recount the sen-

timents or mistakes of others about it: It may suffice to declare my assent in general to the opinion now most commonly received, that the heat of animals is produced by the motion of the blood in the vascular system.

2. Nor does it seem to owe its origin to any intestine motion of the particles of the flowing blood. When I view the circulation in the small pellucid vessels of animals, I see no intestine confusion of particles; nor can I perceive any motion fit for the production of animal heat, except the progressive course of the blood pressing and rubbing upon the sides of the vessels.

3. Nay, I am afraid, that even this motion of the blood, however rapid in the great trunks, will, in the minute vessels, be deemed too small and inconsiderable to produce an attrition sufficient for the generating any sensible heat: So that it may be thought necessary to have recourse to chemical principles; and to suppose heat to be a sort of animal process producing a certain change in the aliments, some way analogous to the luctas and effervescencies we observe many other bodies, in certain cases, to undergo.

4. But it was just now remarked, that we could not perceive any violent intestine motion of the particles of the blood, visible in the ordinary heating effervescencies. And beside, in direct confirmation of the mechanical force of the attrition of the fluids on the sides of the

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the containing vessels, from medical observations we find, in the various circumstances of the human body, the heat generally in some sort to correspond to the degree of motion of the blood. And above all, I hope I shall be able to make it appear, that the vascular system is so admirably contrived, and the various motions of the blood so skilfully adjusted by the great and wise Author of nature, that, bating external influences or disturbances, the heat of the circulating fluids generated by attrition, is every where preserved nearly uniform, with however differing celerities they be propelled in different parts of the animal machine.

5. In order to the clearing and settling of this, I must premise a general supposition, that the intensity of heat generated by attrition is, *ceteris paribus*, in proportion to the relative celerity wherewith the bodies rub against one another.

6. And therefore, if a liquor be forcibly propelled through a canal, the quantity of attrition of the liquor upon the sides of the canal generating heat, is in a compound ratio of the celerity of the liquor (C), and of the circumference (or diameter D) of a section of the canal; that is, always as $C \times D$.

7. When a liquor moves through a canal, its particles, tho' they have no brisk intestine motion, are frequently, however slowly, shifting places. Whence we are to conceive the quantity of attrition around the circumference

of a section, as equally diffused through the whole section, so as to render the heat in every point of it from the circumference to the center perfectly uniform: And therefore the real intensity of heat in such a section is found to be as the quantity of attrition ($C \times D$) applied to (Z) the area of the section, or as $\frac{C \times D}{Z}$; that is as $\left(\frac{C \times D}{D^2}\right)$ the velocity of the liquor, and the diameter of the canal directly, and the square of that diameter reciprocally. Which comes out as $\left(\frac{D}{C}\right)$ the velocity of the liquor applied to the diameter of the canal.

8. And from this it easily follows, that, if liquors be propelled with celerities proportional to the diameters of their containing canals, the heat of these liquors generated by attrition shall be equal. For D, d being the diameters of the canals proportional to C, c the celerities of the propelled liquors; in this case $C \dots c :: D \dots d$, and therefore $C \dots D :: c \dots d$; and consequently $\frac{C}{D} = \frac{c}{d}$; that is, the respective intensities of heat are equal.

9. If a canal carrying a liquor be ramified into branches, the heat of the liquor generated by the lateral attrition may be constantly equal; or may be increased or diminished in its progress through this ramified system of vessels, according

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ing to the method of its divarication, or the proportion that the wideness of the branches bears to the wideness of the trunks from whence they arise.

For the heat of the propelled liquor generated by attrition is every where as its celerity applied to the diameter of the canal through which it is carried; so that if, in the divarication of a canal, the branches have always such a situation and largeness with respect to the trunk from whence they arise, that the velocities of the liquor moving through them shall always correspond to their diameters, then the heat generated by attrition shall be constantly equal.

But, if the largeness of the branches be so adapted to the capacity of the trunks, that the velocity of the liquor flowing in them shall be greater, equal, or even but nearly equal to that in the trunks, then the heat of the liquor generated by attrition would be constantly increasing in its progress through this supposed ramified system, and that, *ceteris paribus*, in a reciprocal proportion of the diameters of the canals.

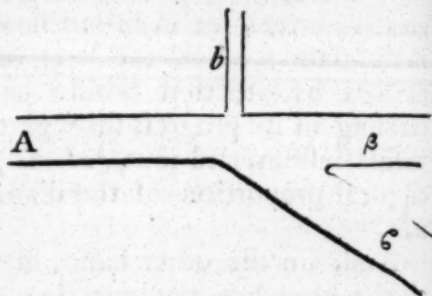
And, on the other hand, if the capacities of all the branches put together should be vastly greater than the largeness of the trunk from whence they arise, so as to diminish the velocities of the liquor in a greater proportion than the diameters of the canals; then indeed the heat in such a system would be constantly on the decay.

10. It seems to be the first of these cases which actually obtains in the animal machine.

For,

For, though the antients made a great ado about the various degrees of heat of the different parts of the human body; yet some of the best and most careful observers amongst the moderns, by methods more certain and regular than what the others were masters of, have been assured that they are all nearly of the same degree of heat, when left to themselves in a sound state, and sufficiently defended from the injuries of the air, or external cold; which I have confirmed by a thousand experiments: And therefore it behoves us to inquire more narrowly into the nature of such a divarication, which may render the velocity of the blood always in proportion to the diameters of its containing canals, and consequently give it a heat constantly equal.

11. Suppose any artery A to be divided into what number of branches b, β, ϵ , you please, equal, or however unequal,



whose diameters we call d, δ, Δ , the diameter of the trunk A being D: So that their respective orifices shall be proportional to $D^2, d^2, \delta^2, \Delta^2$; while the celerities C, c, α, k , wherewith the blood is supposed to flow through these vessels, are required to be proportional to the forefaid diameters D, d, δ, Δ respectively.

Now, in the divarication of any artery, the blood

blood would flow into the several branches with the same, or nearly the same celerity, if they all rose with the same obliquity to the trunk from whence they spring. Therefore it seems necessary, for the due balancing of the velocity, to contrive the divarication in such a manner, that the larger branches shall always lie in a directer course, and the smaller ones in a more inclined one to the current in the trunk, that the quantities of the blood flowing into them may be in a greater ratio than their orifices would otherwise receive; so too that it may flow through them with the requisite velocities proportional to their diameters.

12. Thus then, the position of the branches being settled, we come next to inquire what proportion the amplitude or orifice of the trunk must have to the conjunct amplitudes of the branches, so likewise as to preserve the above mentioned balance of celerity. In which inquiry, in the first place, we are to observe, that, universally, the quantities of liquor passing equably in a given time through any canals, are in a compound ratio of the amplitudes of the canals, and of the velocities of the liquor conjunctly. Hence then the quantities of blood passing in a given time through the branches, b, β, ϵ , are as $d^2 \times c, d^2 \times \alpha, \Delta^2 \times k$ respectively; to all which quantities, the quantity furnished them by the trunk A , or passing through the trunk in the same time, must be equal, and as $D^2 \times C$. That is, then $D^2 \times C = d^2 \times c + d^2 \times \alpha + \Delta^2 \times k$. And therefore, in the present case, (the celerities $C, c,$

C, c, z, k, being supposed proportional to the diameters D, d, δ , Δ) $D^3 \times D$ shall be equal to $d^2 \times d + \delta^2 \times \delta + \Delta^2 \times \delta$; or $D^3 = d^3 + \delta^3 + \Delta^3$. That is, *the cube of the diameter of the trunk is equal to the cubes of the diameters of all its branches added together.* And so D is equal to $\sqrt[3]{d^3 + \delta^3 + \Delta^3}$; that is, *the diameter of the trunk is equal to the cube-root of the conjunct cubes of the diameters of all the branches*

From whence, if once we know what proportion the branches have to one another, we can for certain determine what ratio they, when put together, must have to the trunk from whence they arise. Which was the thing proposed to be found out concerning the nature of that divarication, where the celerities of the flowing liquor are supposed always proportional to the diameters of the containing canals.

13. To illustrate this doctrine of the proportions of the trunks and branches of a system of vessels ramified according to the proposed law of divarication, for preserving the velocities of the contained liquor in a constant ratio to the diameters of its containing canals, and so to maintain a constant uniform degree of heat, let us try the result of it in two or three familiar examples.

If a trunk should divide itself into two equal branches, whose orifices shall be to one another, as 1 to 1, and their diameters in like manner to $\sqrt[2]{1}$ and $\sqrt[2]{1}$, or 1 and 1; then,
by

by the general proposition § 12. the diameter of the trunk is proportional to $3\sqrt{1^3 + 1^3} = 3\sqrt{2} = 1'2599$; and consequently its orifice must be $1'2599^2 = 1'5874$. So that the amplitude of the trunk is to the conjunct amplitudes of the branches as $1'5874$ to $1 + 1 = 2$; or as 100 to 125'99.

Suppose a trunk divided into two, however unequal branches, which, for example, should be to one another as 2 to 1; so that the diameter of the greater branch shall be proportional to $2\sqrt{2} = 1'4141$, that of the lesser as $2\sqrt{1} = 1$. Then the diameter of the trunk is proportional $3\sqrt{1'4141^3 + 1^3} = 1'5644$; and its real largeness, compared to the branches 2 and 1 shall be $1'5644^2 = 2'4473$. So that this amplitude of the trunk, being to the joint amplitudes of the branches as $2'4473$ to $2 + 1 = 3$, shall be to them likewise in the ratio of 100 to 122'586.

But, should the divarication be more complex, so that there be three, and these very unequal branches, in the ratio perhaps of 3, 2, 1; whose diameters therefore must be proportional to $2\sqrt{3} = 1'73205$, $2\sqrt{2} = 1'4141$, and $2\sqrt{1} = 1$: Then the diameter of the trunk comes out proportional to $3\sqrt{1'73205^3 + 1'4141^3 + 1^3} = 3\sqrt{9'02395} = 2'0819$, the square whereof is $4'3344$. Whence we find the trunk to be to the sum of the branches put together as $4'3344$ to $3 + 2 + 1 = 6$; that is, as 100 to 138'42.

14. I observed before, § 10. that the heat in different parts of the body, in a natural and sound state, was every where nearly equal. And now having determined the laws of the divarication of a ramified system of canals wherein the heat generated by attrition should be constantly uniform; let us, in the next place, inquire, if these laws obtain in fact; and if anatomy and a true mensuration of the vessels of the human body do in reality correspond to our theory.

It is obvious at first sight, and every body may have observed, that the position of the vessels intirely favours our scheme. Is it not manifest, as it has been sometimes very justly remarked, that the greatest vessels lie more directly in the way of the trunks from whence they arise, while the small ones rise at very great inclinations according to their various sizes? (the like whereof may, in some measure, be observed even in the ramifications of trees and other vegetables) from whence the blood, flowing more freely into the greater branches, must likewise pass through them with a proportionably greater velocity, than through the minuter vessels, which give not such an advantageous ingress to the pretruded fluid.

15. The next thing we are to consider is the size of the vessels, or the proportions of the branches to their furnishing trunks. It is very difficult, if not impossible, to survey the vessels of animals with a mathematical exactness

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ness, so as to find them always observing the same rule. In measuring an artery, for example, a line broad, who will undertake never to err $\frac{1}{100}$ part of an inch? And yet an error so seemingly little will lead us into a mistake near $\frac{1}{3}$ of the amplitude of the vessel: So that the smallest unavoidable errors in measuring shall occasion some seeming deviations from the rules that really nature may have followed. And therefore I hope even the most scrupulous will be satisfied, if, upon the strictest examination, it shall be found that experience and our theory, though not always jumping exactly in the same numbers, are, however, for the most part, very little wide of one another. Physical experiments and practical mechanics allow only of an approximation to geometrical demonstrations.

Now, it has been observed in general by others, that the conjunct amplitude of the branches of arteries are always larger than the trunks from whence they arise; and consequently that the blood, on this account chiefly, suffers a vast retardation in its progress from the heart to the extreme parts of the body. But we do not stop in this general observation: From innumerable measures and experiments, we moreover pretend to find a determined proportion, and a very elegant harmony in the dilatation of the arterial system, and in the retardation of the blood moving through it; to wit, That the diameter of every artery is equal to the cube-root of the conjunct cubes of the diameters of all its branches: And the velocity of the blood in the arteries always in

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proportion to their several diameters, for the preservation of an equable degree of heat thro' all this far extended system.

16. This, I say, we find from the nicest examination of the vessels that possibly can be made; the several measures sometimes exceeding, sometimes not coming intirely up to the rule; but by so little differences, and these as often on the one side as the other, that we cannot doubt of such small variations having flowed from some overlooked circumstance, or some inevitable errors of mensuration: Which is plain from the following very ample collection of experiments, wherein, at one view, we have the proportions of the trunks to the branches, both according to the theory, and according to the actual measures that were taken of them, with the differences in these two circumstances. Which differences are truly less than we could well have expected; and which would still have been but a half of what we have here set down, if we had made our calculations of the diameters, and not of the amplitudes of the vessels: And they are the diameters which are primarily measured.

17. No body will blame me that I have, in confirmation of this doctrine, chosen to give other people's experiments rather than my own. Those are liable to no objections: The authors of them could have no prepossessions in favour of a scheme they were not apprised of; which I acknowledge would be much more exceptionable, had I founded it only on my own observations.

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In the first place, I have set down some measures, taken with all the care imaginable from the anatomical tables of Eustachio; who, of all the anatomists, seems to have studied, with the greatest accuracy, the symmetry of the human body, and the just proportions of all its parts. However, that I may not conceal any the least material circumstance from you, I must acknowledge, that, in comparing the aorta with the iliacs, I took it where it appears smallest in his figures, which is a little below the emulgents: Which trunk, beside the iliacs, gives off other arteries, as some of the lumbar, the inferior mesenteric, and the sacra. But these, when taken in too, are not really of such consideration as much to disturb or alter our calculations.

18. Then follow some measures taken from Professor Ruysch's figures. His singular art in injecting the vessels, and the great pains he took to have them accurately delineated, every body knows.

But since, whatever Eustachio and Ruysch took, it must have been impossible to shun some small deviations from nature, both in painting and graving their tables, (as we find some variations in the same figures repeated in different tables of Eustachio), and that we likewise may not have measured them with absolute and perfect exactness, you will not wonder that they do not precisely coincide with the numbers of the theory. However, this still comes out as a medium between them.

19. The experiments we have borrowed

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from the ingenious Dr Keill are liable to none of the inconveniencies of painting. He measured the vessels themselves; and therefore I have adopted all his measures: Only some few I have omitted, which, for their being taken from too minute vessels, could not be much relied on; and two or three more, which, by transgressing the necessary laws of ramifications, as omitting a branch or two, or exhibiting a branch greater than its furnishing trunk, discover some mistake to have crept into these observations. On which account, and for other like reasons, I have neglected most of the ordinary anatomical figures: However, even some of these I have now likewise added, under the title of *Miscellany observations*; though I do not pretend to lay so much stress on them, as upon the other experiments I had first taken in, especially those of Keill and Eustachio.

20. That I might not seem to have neglected such measures myself, for the greater confirmation of the rest, if any thing of mine could add to their authority, I have added some of my observations too. And, being unwilling to omit any thing of this kind, in transcribing this paper, I have inserted two or three that the industrious Dr Nichols has now furnished us; who, I find, has been at more than ordinary pains in surveying the vessels both of animals and vegetables.

21. And now follows the collection I promised of the experiments themselves, shewing the proportions of the trunks to their branches in the arterial system of the human body.

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22. In this collection there are indeed but few observations which perfectly jump with the exact proportions of the theory: But many come very near them; and the numbers of those coming short of it, and of those that exceed it, are nearly equal. And the common or middle excess or defect, (reckoning one with another) is only about $\frac{1}{4}$ part. Nay, in about ninety observations, from the excesses and defects balancing one another, the sizes of the branches determined by our rules, and those found out by the most carefully made experiments, come out almost exactly the same. The odds being about $\frac{1}{800}$ or $\frac{1}{1000}$ part; a quantity in such a case to be esteemed as nothing. A surprising coincidence! and which, before trial, we durst not in such difficult and precarious measures have expected or promised upon.

23. Seeing then those observations quadrate so nicely to theory, even more exactly than the most scrupulous would have required, I must acknowledge the very sensible pleasure I had in perceiving this beautiful harmony; and that I was exceedingly delighted to find (as I wished and expected) the arteries of the human body to be distributed and divaricated in such a manner, both in their position and wideness, that the celerity of the blood flowing through them may be preserved in a given proportion to their diameters; and the same, or nearly the same degree of heat generated

by attrition, continued along the whole arterial system.

24. To obviate some difficulties that might be started against us, we must observe, that, for the easier reception of the blood, the artery is always a little wider in its rise from the trunk, from whence it converges in a conical form: And that arteries, considered as trunks, just before they split into branches, must be widened a little for the more convenient divarication. So that, if an artery, from the beginning of its rise to its ramification into branches, happen to be very short, it will commonly be found wider, and have a greater proportion to its branches than our theory would require.

Thus, for example, the great trunk of the right subclavian, before it divides into the carotid and axillary, is sometimes longer, and sometimes shorter. In the first case, as in Eustachio's Tab. XVI. Fig. 1. or Ruysch's Epist. Probi III. Tab. III. Fig. 2. it almost coincides with the theory. In the second case, as in Ruysch's *ibid.* Fig. 3 and Cowper's Anat. hum. bod. Append. Tab. III. it differs so far from our rules, as even to be much larger than the conjoined branches which arise from it. Thus too, the great trunk of the aorta being very short, and also curved, is found to have a greater ratio to its primary branches, than otherwise we should have had reason to expect: And so it seems Valsalva* reckoned it as a
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* See Comment. acad. Bonon. p. 376.

fort of sinus. In which, and other like cases, though we allow the blood to have a slow course through the trunks, we need not be apprehensive of any loss or decay of heat in such short intervals: Which too may soon be sufficiently compensated by the subsequent more regularly adjusted arteries; especially that the smaller vessels do frequently, by anastomoses, communicate one with another; and so, beside other uses, help to maintain a due and regular balance of the velocity of the circulating fluids.

25. And, on the other hand, for the like reasons, we need not be afraid of the heat being too much increased, though it should move something faster in the end, than in the beginning of the long converging arterial trunks, as in the carotids or iliacs; which run a good space before their divarication, without emitting any considerable branches.

26. Contrary to what I alledged of the larger branches rising more directly, and the small ones obliquely to the course of the blood, it will be said that some, even pretty large vessels, rise at an acute angle from their trunks in a retrograde way; such as the umbilical arteries in a foetus, or the epigastrics from the external iliacs. This I acknowledge one will be ready to think from inspecting the figures in the common anatomical tables. But every body that is acquainted with dissections, must have observed, that these tables, either for the convenience of dissecting or painting, or even some-

sometimes, by the carelessness of the authors, do very often exhibit things far out of their natural situation. And, in this particular case before us, if we look into the book of nature itself, and examine things as the great Author thereof has designed them, we shall find, that, though such vessels do carry the blood in a course directly contrary to its flux in the trunks, yet their rise is much in the same angle with others of the like size. Thus the epigastric artery, instead of rising at an acute angle, from the outer side of the external iliac, as Vesalius and the ordinary tables represent it, does really spring from the inner side of that artery (as in Cowper's delineation, *ibid.*) at an angle nearly a right one, but somewhat obtuse; and then forming an arch, climbs upwards, carrying the blood in a course retrograde to what it was in before; which Eustachio * alone has taken care justly to represent, as he is the anatomist in the world who has most carefully studied to exhibit all the parts undisturbed, and in their due and regular posture.

It remains now to apply our rule of the ramification of vessels to some other purposes in the animal oeconomy; and to consider this doctrine with respect to different animals; and to inquire what alterations of heat, if any, their difference of magnitude will produce: How it may be influenced by any other changes,

* Tab. Anat. Tab. XIII. XIV. Fig. 1. Tab. XXV. XXVII. Fig. 12.

changes, whether in the solids or fluids, as by the non-naturals, age, sex, &c. But all this would be difficult to abridge; and to transcribe all, would carry me far beyond the bounds I had fixed to myself at this time.

XII. *An Essay concerning the Motions of our eyes*; by WILLIAM PORTERFIELD, M. D. Fellow of the College of Physicians at Edinburgh.

P A R T I.

OF THEIR EXTERNAL MOTIONS.

THE motions of the eye are either external or internal. I call *external*, those motions performed by its four straight and two oblique muscles, whereby the whole globe of the eye changes its situation or direction. And, by its *internal* motions, I understand those motions which only happen to some of its internal parts, such as the crystalline and iris, or to the whole eye, when it changes its spherical figure, and becomes oblong or flat.

In this paper I shall only treat of its external motions, reserving the internal motions for the far more fertile subject of another essay, which I intend to send you hereafter, if I find that your readers have any relish for such inquiries.

The spherical figure of our eyes, and their loose connection to the edge of the orbit by the *tunica conjunctiva*, which is soft, flexible, and

and yielding, does excellently dispose them to be moved this or the other way, according to the situation of the object we would view. This membrane is from its situation also called *adnata*. It takes its origin from the periorbteum all round the edge of the orbit, and is extended over the whole fore part of the globe, till its termination in the edge of the sclerotis, where it joins the cornea. It is called *conjunctiva*, from its office, *quia oculum cum capite conjungit*.

This membrane is covered externally with another membrane: For, as is known to anatomists, the internal membrane of the eye-lids, at the edge of the orbit, is turned forwards upon the outward face of the eye, and is co-extended over it with the *tunica conjunctiva*, to which it is adherent.

These two membranes, because of their close union, appear to be only one, and are generally described as such under the name of *membrana albuginea*, so called, because they form the white of the eye, though in fact they are distinct membranes, easily to be separated; the one a continuation of the periorbteum, lining the orbits internally, and the other of the inner membrane of the eye-lids. These membranes, especially the external, are so full of blood-vessels, and so laxly extended, that in violent ophthalmia's the white of the eye is sometimes swelled so excessively, as to cover all the cornea, which I here take notice of, because it is ready not only to surprize, but to impose upon the unwary or unexperienced oculist,

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culist, as if it were an incurable excrecence of the cornea itself.

Besides these two membranes, the fore part of the globe is covered all over externally with a very thin transparent aponeurose or surpeau, which not only covers the membrane which it has from the eye-lids, but likewise is extended beyond it over the cornea itself. The phlyctænae, which are small transparent vesicles full of clear water, and which are frequently observed upon the surface of the cornea itself, as well as upon the white of the eye, and even sometimes have their centre in some part of that circle of the cornea, where it joins the sclerotis, and by that means occupy at the same time both a part of the white of the eye, and a part of the cornea, are, amongst other things that might be advanced, a convincing proof of the existence of this surpeau, and of its extension over the whole cornea.

It is by these membranes that the eye is connected to the edge of the orbit, which, being soft and flexible, they do in such a manner, as not in the least to impede its necessary motions. And besides, there is a great deal of fat placed all round the globe, betwixt it and the orbit, which lubricates and softens the eye, and renders its motions more easy.

Now, the external motions of the eye are, as we before hinted, performed by means of six muscles, whereof four are straight, and two oblique. Gabriel Fallopius (in his *Observationes anatomicae*) is among the first that has given us a genuine description of the muscles of the eye: For before him not only Galen but Vesalius

lius himself has grossly erred in the description of the oblique muscles, and in assigning seven muscles to the human eye; on which account Realdus Columbus (*de re anatom. lib. 5. c. 8.*) does indeed justly reprehend them, though, at the same time, he commits no less an error himself, not only in supposing that the *obliquus inferior* begins and ends in the cornea of the eye, but also in imagining, contrary to what Galen and Vesalius teach, that the *obliquus superior* belongs to the eye-lids.

The first of the four straight muscles is situated upon the superior part of the globe upon which it lies. It pulleth up the eye when we look up, and is therefore called *attollens* or *superbus*, it being one of the chief marks of a haughty disposition to look high; wherefore its opposite muscle is called *humilis*. But Casserius Placentinus thinks the motion of the upper eye-lids denotes these dispositions more significantly; for, says he, (*lib. 5. cap. 18.*) "*Qui enim hanc elatam habent*" (speaking of the upper eye-lids) "*superbi et feroces sunt; qui vero depressam ac dimidium fere oculum claudentem, ita ut terram adspicere videantur, humiles et mites sunt.*" For which reason Willis (in his *anima brutorum, cap. 15.*) chuses rather to call them "*pii aut devoti*": Quia in precatione intensa," says he, "*oculum valde attollunt; quare hypocritis, qui sanctitatis speciem affectant, in more est, oculum ita evolvere, ut albo fere tantum conspecto pupilla occultetur.*"

The second, as before hinted, is directly opposite to the *attollens*, and is situated upon the under part of the eye which it pulls down

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down, and is therefore called *deprimens* or *humilis*.

The third and fourth are towards the sides of the eye, and draw it towards the nose, or from it towards the little angle. That which draws it towards the nose is called *abductor* or *bibitorius*, because, in drinking, the eyes are turned inwards to the great angle for viewing the drink. That which pulls it from the nose towards the little angle, is called *abductor*, or *indignabundus*, because it is made use of in those lateral or squint views that denote a scornful resentment.

All these four muscles arise from the circumference of the hole in the bottom of the orbit, through which the optic nerves pass; and, advancing by the four cardinal parts of the eye, terminate by the four broad thin tendons in the sclerotic.

These tendons form a large aponeurose, which is spread over the outward face of the eye under the conjunctiva, to which it also adheres, and terminates at the edge of the sclerotic, where it forms the cornea. Columbus pretends to be the first discoverer of this tunicle, to which he has given no name. Hence it is frequently named *tunica innominata Columbii*, tho' unjustly, because it was known to Galen, as appears from the 2d and 8th chapters of his 10th book *de usu partium*. Others therefore with better reason call it *tunica tendinea*, because formed of the tendons of the four straight muscles. Aquapendent is of opinion, that the white of the eye has its colour from this membrane: But the conjunctiva, and the

tunicle which comes from the inner membrane of the eye-lids, do likewise concur, as has been demonstrated by Plempius (*Ophthalmographia, lib. 1. cap. 8.*).

When the four straight muscles of the eye act separately, they pull the globe up or down, to or from the nose, according to the different situation of objects we would view. But, when the superbus and adductor or abductor act together, or when the humilis and adductor or abductor act together, they perform the oblique motions, which have been attributed to the oblique muscles; and, when all four act together, they draw the eye inwards towards the bottom of the orbit, and keep it fixed in an equal situation, which is therefore by physicians called its *tonic motion*.

Some are likewise of opinion, that when all these four muscles act together, the bulb of the eye is compressed, and its axis is lengthened, when objects are too near us; while others give them a quite contrary action. But this we only mention by the way, reserving it to be further considered when we come to examine the inward motions of this organ.

The oblique muscles of the eye are two in number, whereof one is called *obliquus major* or *superior*, the other *obliquus minor* or *inferior*; they receive their denomination from their oblique position and course.

The *obliquus major*, because of its length, is sometimes called *longissimus oculi*; it arises from the edge of the hole in the bottom of the orbit that transmits the optic nerve, between the elevator and adductor, from whence it runs obliquely

obliquely to the great canthus: In the upper part of which, near the brink, there is a cartilaginous ring or trochlea affixed to the *os frontis*, through which it passes its tendon; from whence turning backwards, it is inserted into the *tunica sclerotica*, towards the back part of the bulb of the eye, in the middle of the distance between the termination of the attollens and the optic nerve.

This trochlea through which this muscle passes its tendon, was first discovered by the great Fallopius, who therefore justly receives the honour due to such a discovery; tho' Riolanus does likewise ascribe it to his contemporary Rondeletius. From it sometimes the muscle receives its name, and is called *trochlearis*: When it acts, it rolls the eye about its axis towards the nose, and at the same time draws it forward, and turns its pupil downwards.

The second of these oblique muscles, because of its being the shortest muscle of the eye, is frequently described under the name of *brevissimus oculi*. It takes its origin from the lower part of the orbit in its inside near its edge; and ascending obliquely by the outer corner of the eye, it is inserted into the sclerotis near the implantation of the former, directly betwixt the abducens and optic nerve.

The action of this muscle is to roll the eye about its axis from the nose, and at the same time to draw it forwards, and direct its pupil upwards.

These two oblique muscles are by some called *circumagentes* and *amatorii* (*amoureux*)

from their actions in winding and rolling the eye about, which motions we call *ogling*. But the French academist Mr Perrault (*du mouvement des yeux*) will not allow that the eyes have ever any motion round their axis, because he could never observe it in the eyes of tortoises, which have some fixed spots that may serve for rendering such motions obvious; but chiefly because he does not see what advantage we could reap therefrom. But, were nature to be confined and limited in her operations by our imperfect views of the advantages of her actions, we should frequently deny the most evident facts in the world.

But that Perrault's authority may not mislead such as have not accurately observed the origin, progress, and insertion of these muscles, it may be proper to observe, that the learned Mr Mariotte (in his *nouvelle decouverte touchant la vue*) has demonstrated beyond dispute, that that part of the bottom of our eyes, where the optic nerves enter them, is insensible; and that the rays of light, which fall thereon, are entirely lost, without giving us any idea of the object from whence they came. Now our optic nerves enter the eye, not in the middle opposite to the pupil, but a little on the inside towards the nose. Hence objects placed a little on the outside of the optic axis, if not over large, would be altogether invisible, because the rays which come from them fall upon that insensible part of the bottom of our eyes, at which the optic nerves enter; but, by the circumrotation of our eyes round their axis, this insensible

part

part may be turned aside, and the rays of light which would have been lost, in falling upon it, may now, at least in part, fall upon the sensible part of our retina; and therefore the object, which otherwise would have been entirely invisible to that eye, may, at least, in part, become visible, which is a considerable advantage, as every one must see.

I am not ignorant that there are many who have denied this oblique insertion of our optic nerves. Willis and Briggs tell us, that not only in man, but also in dogs, cats, and all the more sagacious creatures, they enter the globe at its axis directly opposite to the pupilla: But the labour and industry of later and more accurate anatomists have long ago freed us from this mistake; and, though this obliquity is considerably less in man than in oxen, sheep, swine, and the greatest part of birds and fishes, yet no one, who shall take the pains to examine a human eye, can miss observing it.

There are indeed some creatures, such as the porcupine and sea-calf, that have the optic nerves inserted into the axis of their eyes: Which single fact more effectually overturns Mariotte's hypothesis of the choroides being the principal and immediate organ of sight, than all the subtle reasoning of Messrs Pecquet and Perrault, his greatest opposers. Neither is it possible that this defect in our sight, where the optic nerves enter, can arise from the want of the choroides in this place, which, according to de la Hire's reasoning against Mariotte (see his dissertation, *Sur les*

les differens accidens de la vûe) ought to receive the impression from the rays of light (which, according to him, pass through the transparent retina, without producing vision) and communicate it to the retina, with that disposition and modification which is proper for sight, just as the spiral lamella of the ear receives the impressions of the air, to be communicated to the auditory nerve, for exciting in the mind the idea of sound. For, were this true, then in these just now named animals, all objects would become invisible, to which their eyes are directly turned, because the choroides is wanting in that place where their image falls; which being contrary to experience, it remains that some other cause be assigned for that defect of our sight than the want of the choroides. But, to return:

Tho' the action of these two oblique muscles seems pretty evident, there is scarce any part of the human body about which anatomists have differed more, than in assigning them their proper offices. The famous Mr Cowper is among the first I know who began to reason justly about them. But it would take up too much time to enumerate and confute the several opinions of different authors; and therefore I shall content myself, after what has been already said of each muscle acting a part, to consider what happens, when both act at the same time.

Mr Cowper, in his *Myotomia reformata*, has well observed, that when any of the straight muscles act, they will rather draw the eye inwards, within the orbit, than turn it either sideways,

sideways, or upwards, or downwards, were it not at the same time drawn outwards by some equal force. Now, the above described situation of these oblique muscles excellently qualifies them for keeping the globe from being retracted, when any of its straight muscles act: For, by their joint contraction, they must pull the eye outward from the bottom of the orbit, and keep it suspended as upon an axis, for the better receiving the motions of the straight muscles: And this is what we think the principal use of its oblique muscles, when acting together, seeing they combine both in this, while they are antagonists to one another in their other actions.

Aquapendent (in his treatise *de oculo*, cap. xi.) observes, that, in the pike, the oblique muscles decussate one another in form of a cross; and Perrault (*du mouvement des yeux*) tells us, that they are both in the under part of the eye; and that because, in such rapacious animals, who frequently dive in pursuit of their prey, they have occasion more than others to turn their eyes downwards. But this we chiefly take notice of, because it may afterwards be of some use for determining how the eye changes its conformation, and adapts itself to the different distances of objects, which some have ascribed to the action of those muscles.

Cowper (in his *Myotomia reformatæ*) quotes Mullinette, for describing a seventh muscle, which he calls the fifth right muscle, whose office he confines to the motion of the trochlea. But, upon examination, no such muscle
is

is to be found in the human eye; and it is possible that Mullinette might be led into the mistake, by that part of the *orbicularis palpebrarum*, which adheres to the trochlea, or rather by what he might have observed in dogs, who have a small muscle arising near the origin of the *obliquus major*, and inserting itself by a very slender tendon into the trochlea, to whose motions it is subservient, as Douglas observes (*Myographia comparata, cap. vi.*)

Besides these muscles already described, quadrupeds are provided with another, commonly called *suspensorius*, from its assigned use in suspending the eyes of such animals as go much with their head hanging down towards the ground. This muscle, among other things, discovers that Vesalius has not been altogether free from a fault, which he condemns very severely in Galen, to wit, the obtruding on us the organs of brutes, instead of those of the human body, which he pretends to describe; for he has both described and painted it as belonging to man, in whom it is never found.

This muscle arises from the circumference of the hole in the bottom of the orbit through which the optic nerve passes, and goes directly along the optic nerve, which it embraces and surrounds on all hands, and is inserted into the back part of the sclerotis, all round the optic nerve, betwixt it and the termination of the straight muscles. Fishes and fowls commonly want this muscle, as well as man; but oxen, horse, sheep, hogs, and, so far as has been observed, all quadrupeds, are provided therewith, though, in all, it is not of the same structure,

structure being sometimes composed of two, three, or four distinct muscles, as Aquapendent (*de oculo, cap. xi.*) observes.

Aquapendent, Willis, and Briggs, with the greatest part of our modern anatomists, are of opinion, that the only use of this muscle, is to draw the eye inwards, towards the bottom of the orbit, and to keep it suspended, that, when the eye hangs down, as often happens in quadrupeds who gather their food from the ground, it may not fall too much out of the orbit, or by its weight stretch and fatigue the optic nerve to which it is attached. Hence they call it *suspensorius*, as has been before observed. But this action may in part be supplied by the straight muscles acting together; and, besides, a ligament would have been sufficient for suspending the eye; and therefore it is probable that this muscle has some other use.

Dr Tyson, finding this muscle in the porpess, as well as in quadrupeds, thinks its use is not to suspend the bulb of the eye, but rather, by its equal contraction of the sclerotis, to which it is affixed, to render the ball of the eye more or less spherical, according to the different distances of objects, concerning which you may consult his *Anatomy of the porpess*, (p. 39.) But it is not absolutely certain that the figure of the eye can be changed by the action of this muscle, and that for reasons afterwards to be mentioned, when we come to consider its internal motions; and besides, the necessary change of our eyes is well provided for by another mechanism, as will also appear in its proper place.

I think therefore that the use of this muscle is not only to suspend the eye, and preserve the optic nerve from being too much stretched, but principally to assist the straight muscles in moving the eye, according as its different fibres act, *e. g.* when its superior fibres act, they assist the attollens in pulling the eye up; when its internal fibres next the nose act, they assist the adducens; and, when both together, or those betwixt them, act, they pull the eye obliquely upwards towards the nose, and consequently assist the attollens and adducens in their joint action of moving the eye obliquely. Comparative anatomy makes this opinion very probable; for, in several animals, as we have before hinted, it is divided into several distinct muscles, whereof Aquapendent has observed sometimes three, and sometimes four, in the eyes of sheep; and Douglas tells us, That in a dog it is divided sometimes into four, and sometimes into five, which have as many distinct insertions into the sclerotis. Mr Perrault's observation on this muscle does likewise very much confirm this opinion. (See his treatise, *Du mouvement des yeux*). His words translated are: "In effect we may say, (speaking of this muscle), that it contributes to the action of the straight muscles, according as its fibres act differently, there being several creatures, such as the bear, pole-cat, (*Pours, la Fouine*), and many others, where this muscle is separated into four, having as many different insertions, which being betwixt the insertions of the four straight muscles, may serve for the oblique motions of the eye, which

" which in man are chiefly performed by the
 " combination or successive action of the four
 " straight muscles."

Having examined what belongs to the mechanism of the external motions of our eyes, I shall now beg leave to add some reflections thereon, which I flatter myself will not be altogether unacceptable to some of your readers. And,

I. When nature has denied the head or eyes any motion, it is to be observed that she has with great care and industry provided for this defect. Dr Power's microscopical observations furnish us with a beautiful example of this: His words are: (Observat. 8.) " The
 " first eminent thing we found in the house-
 " spiders were their eyes, which in some were
 " four, in some six, and in some eight, according to the proportion of their bulk and
 " the longity of their legs. These eyes are
 " placed all in the forefront of their head,
 " (which is round and without any neck) all
 " diaphanous and transparent like a locket of
 " diamonds, or a set of round crystal-beads,
 " &c. Neither wonder why Providence should
 " be so anomalous in this animal more than
 " in any other we know of, (Argus's head
 " being fixed to Arachne's shoulders :) For,
 " 1st, Since they, wanting a neck, cannot
 " move their head, it is requisite that defect
 " should be supplied by the multiplicity of
 " eyes. 2^{dly}, Since they were to live by catching so nimble a prey as a fly is, they ought
 " to see her every way, and to take her *per*
 " *saltum* (as they do) without any motion of
 " their

“their head to discover her; which motion
 “would have scarred away so timorous an in-
 “sect.”

It is therefore with good reason that Muffet, speaking of this Lydian spinstrefs, that proud madam, whom, for her rivalship, the fable makes Pallas transform into a spider, says of those philosophers that held them blind, “Sane
 “cœcutiunt illi summo meridie, qui videre ip-
 “sas non vident neque intelligunt:” Which he might have said with far better reason, if his eyes had been but assisted with one of our common microscopes.

To this purpose also belongs the surprisingly beautiful and curious mechanism observable in the immoveable eyes of flies, wasps, &c. they nearly resemble two protuberant hemispheres, each consisting of a prodigious number of other little segments of a sphere; all which segments are perforated by a hole which may be called their *pupil*, in which this is remarkable, that every foramen or pupil is of a lenticular nature, so that we see objects through them topsy-turvy, as through so many convex glasses; yea they become a small telescope, when there is a due focal distance between them and the lens of the microscope. Leuwen-hœck's observations make it probable that every lens of the cornea supplies the place of the crystalline humour, which seems to be wanting in those creatures, and that each has a distinct branch of the optic nerve answering to it, upon which the images are painted; so that as most animals are binocular, and spiders for the most part octonocular, so flies, &c. are mult-

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The divaricated ARTERIES.

From <i>EUSTACHIO</i> .			Proportions of the Branches.	Capacities of the Trunks.	Conjunct Capacities of all the Branches.		Differences.
					By the Theory.	By mea- suring.	
The right Subclavian artery, divided into the axillary and carotid, tab. xvi. fig. 1.	-	-	11 . 12	100	126	129	$\frac{1}{45}$ +
A Mesenteric artery, the exactest way I could take the measures, tab. xi. fig. 1.	-	-	8 . 5	100	124	122	$\frac{1}{85}$ -
A Mesenteric artery in another subject, tab. xxvii. fig. 4.	-	-	1 . 1	100	126	119	$\frac{1}{18}$ -
The descending aorta split into the two	{	tab. xii. fig. 1.	1 . 1	100	126	128	$\frac{1}{85}$ +
Iliacs,	-	{ tab. i. fig. 3.	1 . 1	100	126	134	$\frac{1}{10}$ +
The same in another subject,	-	{ tab. xii. fig. 1.	1 . 1	100	126	130	$\frac{1}{35}$ +
	-	{ tab. i. fig. 2.	1 . 1	100	126	123	$\frac{1}{45}$ -
The same in another,	-	{ tab. xii. fig. 4.	1 . 1	100	126	120	$\frac{1}{15}$ -
	-	{ tab. iv. fig. 5.	1 . 1	100	126	120	$\frac{1}{15}$ -
The same in another,	-	{ tab. xii. fig. 7.	1 . 1	100	126	146	$\frac{1}{8}$ +
	-	{ tab. i. fig. 3.	1 . 1	100	126	132	$\frac{1}{15}$ +
The same in another,	-	{ tab. xii. fig. 9.	1 . 1	100	126	138	$\frac{1}{15}$ +
	-	{ tab. iii. fig. 1.	1 . 1	100	126	136	$\frac{1}{15}$ +
The same in another,	-	{ tab. xii. fig. 10.	1 . 1 q. p.	100	126	136	$\frac{1}{15}$ +
	-	{ tab. iii. fig. 3.	1 . 1 q. p.	100	126	123	$\frac{1}{45}$ -
The same in another,	-	{ tab. xii. fig. 12.	1 . 1	100	126	114	$\frac{1}{15}$ -
	-	{ tab. iii. fig. 3.	1 . 1	100	126	110	$\frac{1}{5}$ -
The same in another, tab. ii. fig. 1.	-	-	1 . 1	100	126	131	$\frac{1}{35}$ +
The same in another, tab. ii. fig. 2.	-	-	1 . 1	100	126	138	$\frac{1}{15}$ +
The same in another, tab. ii. fig. 3.	-	-	1 . 1	100	126	133	$\frac{1}{18}$ +
The same in another, tab. xxv.	-	-	1 . 1	100	126	121	$\frac{1}{35}$ -
The same in a woman, tab. xiii.	-	-	1 . 1	100	126	124	$\frac{1}{85}$ +

From *RUYSCH*.

The right Subclavian branched into the axillary and carotid, Ep. probl. III. tab. iii. f. 2.	-	-	2 . 1	100	123	136	$\frac{1}{10}$ +
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Place this and the following tables fronting page 144. vol. 3.

The divaricated ARTERIES.

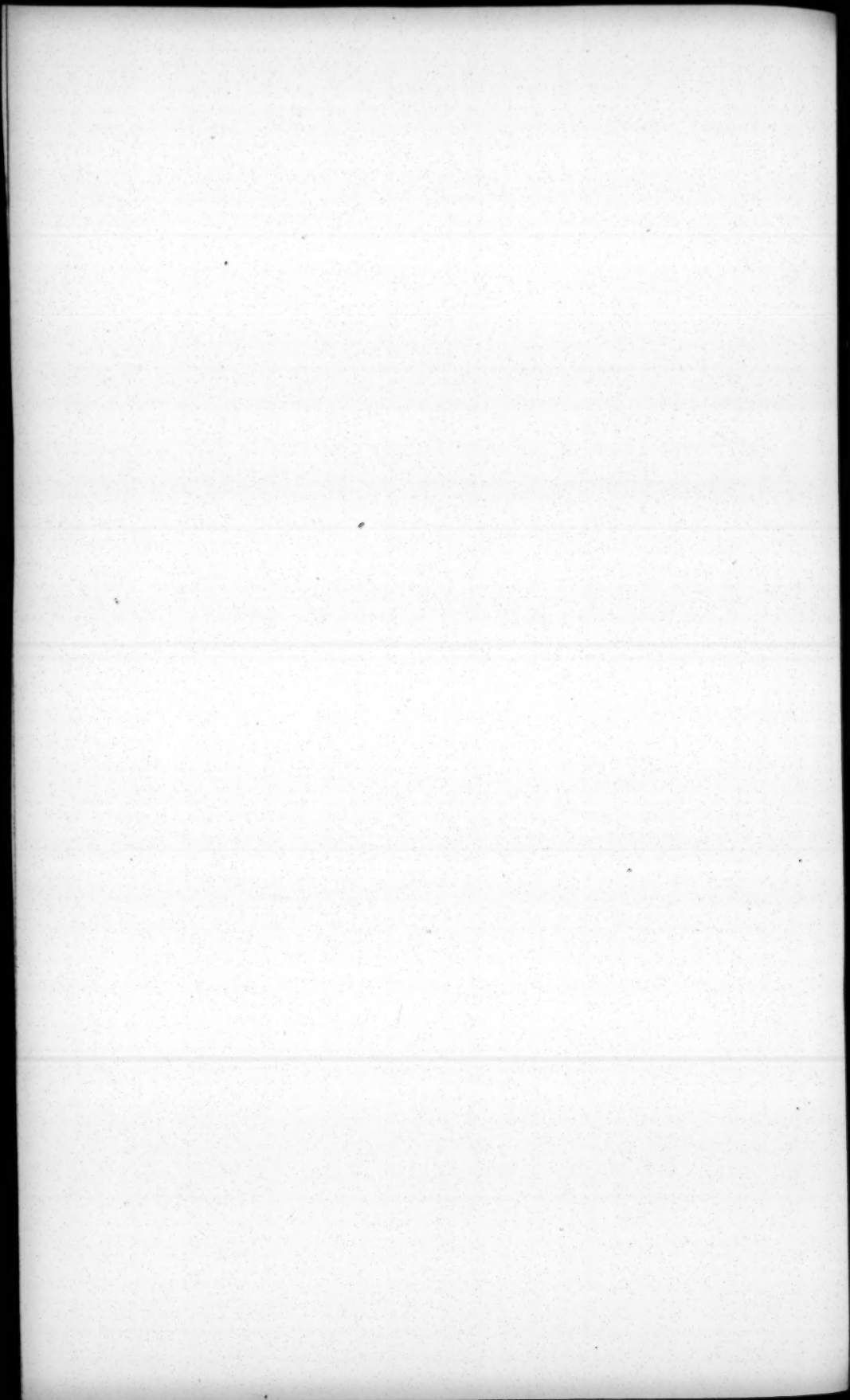
	Proportions of the Branches.	Capacities of the Trunks.	Conjunct capacities of all the Branches.		Differences.
			By the Theory.	By measuring.	
The superior branch of the splenic artery entering the spleen, } Ep. probl. iv. tab. iv. fig. 2. - - - - - }	4 . 4 . 1	100	136	125	$\frac{1}{12}$ -
Its inferior branch, ibid. - - - - -	3 . 2	100	125	121	$\frac{1}{12}$ -
The uppermost artery from this inferior branch, ib. - - - - -	7 . 6	100	126	130	$\frac{1}{12}$ +
The lower one, ib. - - - - -	5 . 4	100	126	140	$\frac{1}{12}$ +
A mesenteric artery, Mus. anat. p. 76. fig. 5. - - - - -	1 . 1 q. p.	100	126	128	$\frac{1}{12}$ +
Its right branch, ib. - - - - -	10 . 9 q. p.	100	126	136	$\frac{1}{12}$ +
Its left branch, ib. - - - - -	5 . 1	100	113	108	$\frac{1}{12}$ -
From KEILL.					
The superior mesenteric artery, spending itself in twenty } one branches, tent. iv. p. 88. - - - - - }	2136, &c.	100	258	247	$\frac{1}{12}$ -
The 5th branch of the mesenteric, ib. p. 90. l. 15. - - - - -	5 . 8	100	124	102	$\frac{1}{12}$ -
The larger branch of this 5th mesenteric, ib. l. 25. - - - - -	5 . 9 . 8	100	142	150	$\frac{1}{12}$ +
The 3d twig of this larger branch, p. 91. l. 4. - - - - -	4 . 3 q. p.	100	125	153	$\frac{1}{12}$ +
The 1st branch of the 8th mesenteric, ib. l. 12. - - - - -	1 . 1	100	126	118	$\frac{1}{12}$ -
The 2d branch of the 8th mesenteric ib. l. 16. - - - - -	4 . 5	100	126	133	$\frac{1}{12}$ +
The 10th mesenteric artery, ib. l. 20. - - - - -	6 . 5	100	126	117	$\frac{1}{12}$ -
The first branch of the 10th mesenteric, ib. l. 24. - - - - -	5 . 6	100	126	123	$\frac{1}{12}$ -
The 2d twig of this 1st branch, p. 92. l. 3. - - - - -	1 . 1	100	126	105	$\frac{1}{12}$ -
The 14th mesenteric ib. l. 17. - - - - -	1 . 1 . 1	100	146	144	$\frac{1}{12}$ -
The 15th mesenteric, ib. l. 12. - - - - -	4 . 5	100	126	127	$\frac{1}{12}$ +
The 2d branch of this 15th mesenteric, ib. l. 16. - - - - -	7 . 6 . 6	100	144	156	$\frac{1}{12}$ +
One of the twigs of this second branch, ib. l. 21. - - - - -	3 . 4	100	125	137	$\frac{1}{12}$ +
The femoral artery dividing into two branches, Tentam. } II. p. 45. l. 2. - - - - - }	5 . 4 q. p.	100	126	157	$\frac{1}{4}$ +
Its 2d branch, ib. l. 3. - - - - -	3 . 5 q. p.	100	124	113	$\frac{1}{12}$ -
The 2d branch in l. 3. ib. l. 4. - - - - -	4 . 2 . 1	100	135	157	$\frac{1}{6}$ +
The 1st branch in l. 4. ib. l. 5. - - - - -	2 . 1	100	123	136	$\frac{1}{10}$ +
The 2d branch in l. 5. ib. l. 6. - - - - -	11 . 5	100	122	128	$\frac{1}{20}$ +

The divaricated ARTERIES.

	Proportions of the Branches.	Capacities of the Trunks.	Conjunct Capacities of all the Branches.		Differences.
			By the Theory.	By mea- suring.	
The 1st branch in l 6. ib. l 7.	5.3.1 <i>q. p.</i>	100	135	114	$\frac{1}{10}$ —
The 1st branch in l 7. ib. l 8.	3.2	100	124	124	0 —
The 1st branch in l 11. ib. l 9.	2.9	100	115	107	$\frac{1}{21}$ —
A branch of the femoral artery in its progress, ib. l 10.	3.2.30 <i>q. p.</i>	100	109	113	$\frac{1}{18}$ +
The 3d branch in l 10. ib. l 11.	1.5	100	113	112	$\frac{1}{11}$ +
The 1st branch in l 13. ib. l 14.	2.1	100	123	144	$\frac{1}{10}$ +
The 1st branch in l 14. ib. l 15.	5.6	100	126	120	$\frac{1}{15}$ —
The 2d branch in l 9. ib. l 16.	1.8 <i>q. p.</i>	100	109	87	$\frac{1}{12}$ —
The 2d branch in l 16. ib. l 17.	1.1.4	100	129	149	$\frac{1}{7}$ +
The 3d branch in l 17. ib. l 18.	3.2.30	100	109	109	0 —
The 2d branch in l 5. ib. l 19.	10.9	100	126	122	$\frac{1}{11}$ —
From my own Dissections.					
The right subclavian branched into the axillary and carotid, } in a man,	3.2	100	125	111	$\frac{1}{9}$ —
The same in a woman,	4.3	100	125	124	$\frac{1}{12}$ —
The same in a young girl,	8.9	100	126	119	$\frac{1}{18}$ —
The right carotid divided into the internal and external in } a man,	3.2	100	125	127	$\frac{1}{61}$ +
The same in a girl,	7.6	100	126	107	$\frac{1}{7}$ —
The aorta ending in the iliacs in a boy,	1.1	100	126	106	$\frac{1}{8}$ —
The iliac artery branched into the external and internal in } a man,	5.4	100	126	128	$\frac{1}{61}$ +
The same in a young man,	5.4	100	126	130	$\frac{1}{12}$ +
The same in a boy,	5.4	100	126	118	$\frac{1}{11}$ —
The same in another boy,	5.4	100	126	130	$\frac{1}{12}$ +
The same in another boy,	10.9	100	126	129	$\frac{1}{11}$ +
The same in a woman,	1.1	100	126	124	$\frac{1}{61}$ —
The same in a young girl,	5.4 <i>q. p.</i>	100	126	132	$\frac{1}{11}$ +
The same in another young girl,	5.4	100	126	131	$\frac{1}{12}$ +

The divaricated ARTERIES.

The divaricated ARTERIES.	Proportions of the Branches.	Capacities of the Trunks.	Conjunct capacities of all the Branches.		Differences.
			By the Theory.	By measuring.	
From NICHOLS.					
An artery A (which I take to be one of the mesenterics) ramified into the branches B, C, Comp. anat. tab. ii. p. 2. }	3.4	100	125	113	$\frac{1}{11}$ -
The branch C, divided into D, E, -	1.2	100	123	116	$\frac{1}{17}$ -
The branch E divided into the smaller branch 36, and another (Z) which I find to be about 21, - }	5.3	100	124	116	$\frac{1}{13}$ -
The branch (Z) ramified into the twigs 16 and 9, -	5.3 q. p.	100	124	119	$\frac{1}{15}$ -
Miscellany Observations.					
A branch A of the external carotid, divided into the anterior and posterior branches C, B, Du Verney Org. de l'Ouie, tab. ii. fig. 1. }	5.3 q. p.	100	124	100	$\frac{1}{5}$ -
The same, ib. fig. 2. -	4.3 q. p.	100	125	113	$\frac{1}{10}$ -
The anterior branch, after throwing off D, subdivided into two branches, ib. fig. 1. }	2.1 q. p.	100	123	133	$\frac{1}{11}$ +
The descending aorta, ending in the inferior mesenteric, Lumbar and iliacs, Verheyen, anat. tab. xvi. fig. 1. }	3.1.1.40 40 q. p.	100	132	134	$\frac{1}{68}$ +
The aorta ending in the iliacs, the inferior mesenteric, 4 Lumbar and 3 Sacrae, Cowper anat. hum. Bod. app. tab. iii. }	23.26.2 1.1, &c.	100	140	124	$\frac{1}{9}$ -
The right iliac into the external and internal, ib.	1.1	100	126	106	$\frac{1}{10}$ -
The left iliac divided the same way, ib. -	1.1	100	126	105	$\frac{1}{10}$ -
The cæliac into 2 branches, Chefelden anat. tab. xvii. 2.	3.2 q. p.	100	125	132	$\frac{1}{18}$ +
The larger branch of the cæliac into lesser branches, ib.	9.9.5	100	142	144	$\frac{1}{11}$ +
The superior mesenteric divided into three branches, ib. 3.	5.6.1	100	129	130	$\frac{1}{119}$ +
The inferior mesenteric divided into three branches, ib. 4.	11.20.11	100	121	133	$\frac{1}{11}$ +
The descending aorta ending in the iliacs, ib. tab. xv.	1.1	100	126	121	$\frac{1}{11}$ -
The cæliac artery branching into the right and left Stukely of the spleen, tab. i. }	9.10	100	126	105	$\frac{1}{8}$ +
A capillary artery of the intestines sending off eight equal branches, Hales Hæmostat. ix. 14. p. 53. }	1.1.1 1.1.1.1.1	100	200	200	0



multocular, having, in effect, as many eyes as there are perforations in the cornea. By which means, as other creatures, but with two eyes, are obliged, by the contraction of the muscles above described, to turn their eyes to objects, these have some or other of their pupils always ready placed towards objects nearly all round them; whence they are so far from being denied any benefit of this noble and most necessary sense of sight, that they have probably more of it than other creatures, answering to their necessities and way of living. And thus provident nature has with great industry and art provided for the immobility of the head and eyes.

II. As in man and most other creatures, the eyes are situated in the head, because, amongst other reasons, it is the most convenient place of their defence and security, being composed of hard bones, wherein are formed two large strong sinuses or sockets, commonly called orbits, for the convenient lodging of these tender organs, and securing them against external injuries; so in those creatures, whose head, like their eyes and the rest of their body, is soft and without bones, nature hath provided for this necessary and tender organ a wonderful kind of guard, by enduing the creature with a faculty of withdrawing its eyes into its head, and lodging them in the same safety with its body. We have a very beautiful example of this in snails, whose eyes are lodged in their four horns, like atramentous spots, one at the end of each horn, which they can retract at pleasure when in any danger.

ger. I know the learned Perrault (in his *mechanique des animaux*) seems to doubt of snails having eyes: And Dr Brown ranks this conceit of the eyes of snails amongst the vulgar errors of the multitude; but a good microscope would soon have shewn him his own error. Those that desire further satisfaction in this particular, may consult Dr Power's observations, and *Lister de Cochleis & Limacibus*.

If it should be here asked, Whence it is that fishes, whose eyes are not guarded and defended by eye-lids, should not also have a power of retracting their eyes for their defence and security? To which I answer, That, if we reflect on the hardness of the cornea, which, in all animals that want eye-lids, exactly resembles the horn of a lanthron, and therefore is not to be hurt by such particles as their eyes are commonly exposed to, we must see that such a mechanism would have been useless: And beside, in some crustaceous animals, whose occasions and manner of living, perhaps, expose their eyes to greater dangers and inconveniencies, their eyes are well secured by deep sinuses, into which, as into a safe chamber, they can retract their eyes upon the approach of any danger, as has been well observed by *Fabricius ab Aquapendente* (in his treatise *de Oculo*, cap. 14).

Something of a mechanism similar to this has also been thought to obtain in the eyes of moles, which are not blind, as Aristotle, Pliny, Severinus, &c would persuade us; but, being provided with little black eyes, about the bigness of a small pin-head, in which not only
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the aqueous, vitreous, and crystalline humors, but also the *ligamentum ciliare*, copped or conical cornea, with the round pupil and optic nerve, have been manifestly discerned, they must necessarily serve to guide and secure it, when it chances to be above ground. But, because this animal lives most under ground, which it digs and penetrates, it was necessary their eyes should be well guarded and defended against the many dangers and inconveniencies to which their manner of living exposes them; and this is the reason why their eyes are so small, and that they are situated so far in the head, and covered so strongly with hair, that they can be of no service to them, unless they be possessed of a power of protruding and retracting them at pleasure, more or less as they have more or less occasion to use or guard their eyes, as has been observed by Borrichius, *Epist. Bartholin.* 92. cent. iv. Mr Derham's *Physico-theology*, book iv. chap. 2. &c.

III. The third and last reflection we shall make upon the motion of our eyes, is what regards a problem which has very much perplexed both physicians and philosophers, *viz.* What is the cause of the uniform motion of both eyes?

In some creatures, such as fishes, birds; and, among quadrupeds, the hare, camelion, &c. the eyes are moved differently, the one towards one object, and the other towards another: But in man, sheep, oxen, and dogs, the motions are so uniform, that they never fail to turn both towards the same place. Hence, in operations upon the eye, that require it to be kept immove-

able for some time, it is necessary to tie up the sound eye with compress and bandage, by which means the other is easier kept fixed and immoveable.

The final cause of this uniform motion of our eyes is,

1. That the sight may be thence rendered more strong and perfect; for since each eye apart impresses the mind with an idea of the same object, the impression must be more strong and lively when both eyes concur, than when only one; and consequently the mind must receive a more strong, lively, and perfect idea of the object in view, as is agreeable to experience: And that both may concur, it is necessary they move uniformly; for, though the retina, or immediate organ of vision, be expanded upon the whole bottom of the eye as far as the *ligamentum ciliare*, yet nothing is distinctly and clearly seen but what the eye is directed to. Thus, in viewing any word, such as MEDICINE, if the eye be directed to the first letter M, and keep itself fixed thereon for observing it accurately, the other letters will not then appear clear or distinct, because the several pencils of rays that come therefrom fall too obliquely on the crystalline and other humors of the eye, to be accurately collected in so many distinct points of the retina; and chiefly because of a certain degree of hardness, callosity, or insensibility, that obtains in all parts of the retina, excepting towards the axis of the eye, directly opposite to the pupil. Hence it is, that to view any object, and thence to receive the strongest and most lively impressions,

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it is always necessary we turn our eyes directly towards it, that its picture may fall precisely upon this most delicate and sensible part of the organ, which is naturally in the axis of the eye. But if this most sensible and delicate part happen, from a fault in the first conformation, or from any other cause, not to be in the optic axis, but a little off at a side, then, to see an object clearly, the eye must not be directed towards it, but a little to a side, that its picture may fall on this most sensible part of the organ: And this may be one cause of squinting, which, as is easy to see, must be altogether incurable.

Now, though it is certain that only a very small part of any object can at once be clearly and distinctly seen, namely, that whose image on the retina is in the axis of the eye; and that the other parts of the object, which have their images painted at some distance from this same axis, are but faintly and obscurely perceived, yet we are seldom sensible of this defect; and, in viewing any large body, we are ready to imagine that we see at the same time all its parts equally distinct and clear: But this is a vulgar error, and we are led into it from the quick and almost continual motion of the eye, whereby it is successively directed towards all the parts of the object in an instant of time; for it is certain, that the ideas of objects, which we receive by sight, do not presently perish, but are of a lasting nature, as appears from what happens when a coal of fire is nimbly moved about in the circumference of a circle, which makes the whole circumference

cumference appear like a circle of fire. because the idea of the coal, excited in the mind by the rays of light, are of a lasting nature, and continue till the coal of fire, in going round, return to its former place; and therefore, if our eye takes no longer time to direct itself successively to all the small parts of an object, than what the coal of fire takes to go round, the mind will distinctly perceive all those parts, without being sensible of any defect or insensibility in any part of the retina, because the idea of one part continues, till, by the motion of the eye, the image of the other parts be successively received upon the same most sensible part of the retina: And this is the reason why the globe of the eye moves so quickly, and that its muscles have such a quantity of nerves to perform their motions. But I go on.

2. A *second* advantage we reap from the uniform motions of our eyes, which is yet more considerable than the former, consists in our being thereby enabled to judge with more certainty of the distance of objects.

There are six means which concur for our judging of the distance of objects, of all which the most universal, and frequently the most sure, is the angle which the rays of light make at the object in coming thence to our eyes. When this angle is very great, we see the object very near; and, on the contrary, when it is very small, we see it at a great distance; and the change which happens in the situation of our eyes, according to the change of this angle, is a mean which our mind makes

makes use of for judging of the distance and proximity of objects. To be persuaded of the truth of this, suspend by a thread a ring, so as its side may be towards you, and its hole look right and left, and taking a small rod, crooked at the end, in your hand, retire from the ring two or three paces, and having with one hand covered one of your eyes, endeavour with the other to pass the crooked end of your rod through the ring. This appears very easy, and yet, upon trial, perhaps once in a hundred times you shall not succeed, especially if you move the rod a little quickly. This surprising difficulty, which is found in passing the rod, arises, because, when one eye is shut, the angle which the rays of light make at the object, in coming thence to both eyes, is not known; for, in any triangle to know the bigness of an angle, it is not sufficient to know the length of the base subtending that angle, and the magnitude of the angle which one of its sides make with that base, as is known to mathematicians, but it is also necessary to know the other angle which the other side makes with the base: But this can never be known but in opening both eyes, and directing them to the object; and therefore the mind can never make use of its natural geometry, for judging of the distance of the ring, when one of the eyes is shut.

From this we may see the use of having two eyes placed at a certain distance from one another; for, by use, we get a habit of judging of the distance of objects by the direction of the axes, which is sensible to us, because it depends

pende on the motion of the eye that we feel. But other creatures that look differently with their eyes, as fishes, fowls, the hare, camelion, &c. cannot judge of the distance of objects from this angle, and therefore must be more liable to mistakes than we are; yet nature has provided them with two eyes, that their sight might not be too much limited, but that they might see objects equally well on both sides, and thereby be better enabled to seek their food, and avoid dangers: Whence it is, that in some animals they are seated so as to see behind them, as well as on each side. We have a very remarkable example of this in hares and conies, whose eyes are very protuberant, and placed so much towards the sides of their head, that their two eyes take in nearly a whole sphere; whereas, in dogs, that pursue them, the eyes are set more forward in the head to look that way more than backward.

From this also we may see, why we err so frequently in the judgments we form of the magnitude of objects seen only with one eye: For, since we judge not of the extension or magnitude from the apparent magnitude alone, but also from the apparent distance, it follows, that objects seen with one eye must appear smaller or greater, as they are imagined nearer or further off. Thus, a planet viewed with a telescope, sometimes is judged near the eye-glass; and therefore appears very small, while to others it appears very great, because imagined a good way beyond the objective. The same thing happens in viewing one's self in a great concave mirror not too far off; when the

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one eye is shut, the face does not appear very big, because it is imagined at no greater distance than the surface of the mirror; but to both eyes it appears a great deal bigger, because it is then imagined much farther off, as has been observed by Mr Mariotte (*Traite des couleurs*).

It being therefore manifest, that the disposition of our eyes, which always accompanies the angle formed of the visual rays that flow to both pupils, and that cut one another in that point of the object on which our eyes are fixed, is one of the best and most universal means we have for judging of the distance of objects; it needs be no surprize, that in very great distances, where the distance of our eyes bears no sensible proportion to the distance of the object, it should be impossible for us, by this or any other method, to judge rightly of the distance, because the change that happens here to this angle is so small, as to be altogether insensible.

Every body must see that this angle changes considerably, when an object that is only a foot from our eyes, is transported to four; but if from four it be transported to eight, the change is by much less sensible; if from eight to twelve, it is yet less; if from a thousand to an hundred thousand, it is scarce any more sensible, nay, not though the distance be increased from a thousand to an infinite space.

It is for this reason that we are so often deceived in the judgment we form of all great distances, and that we see the sun, moon, and stars, as if they were involved in the clouds, though

though it is certain they are vastly beyond them. And, being deceived as to their distance, we must also be deceived with respect to their magnitude. Thus the moon seems greater than the greatest star, though every body knows that she is vastly less. Thus the sun and moon appear not above a foot or two in diameter, if we trust the testimony of our eyes, as did Epicurus and Lucretius, who therefore imagined them no bigger than what they appeared. Thus also the sun and moon appear greater when near the horizon, than at a greater height, because, when nigh the horizon, they are judged at a greater distance.

There is yet another advantage full as considerable as any of the former, that is thought to arise from the uniform motion of our eyes, and that is, the single appearance of objects seen with both eyes.

This indeed, at first view, does appear very probable; for if, in looking to any object, you press one of your eyes aside with your finger, and alter its direction, every thing will be seen double, which is a common experiment where-with children amuse themselves, being delighted with the uncommon double appearance of objects.

The same thing also happens, when either of the eyes is, from a spasm or paralysis of any of its muscles, or from any other cause, restrained from following the motions of the other. Thus Willis (in his *Anima brutorum*, cap. 15.) tells us of a young man, long ill of the palsy, who at last came out to see all things double, from a spasm in the adducent muscle of his left eye,

eye, whereby its axis was turned inwards, so that it could not be directed to the same object with the other.

Platerus likewise (in the first book of his Observations, p. 132.) gives us the history of a boy, who, after having receiving a stroke on his head, became paralytic in one of his sides, and had his mouth distorted; to whom every thing he looked at appeared double: And tho' he does not attempt to account for this depravation of sight, yet it is easy to see that it could proceed from nothing but a palsy or spasm of one of the muscles of one of his eyes, by which it was rendered incapable of following the motion of the other.

Langius also has a very remarkable case to this purpose, which, being a little uncommon, we must not omit. He tells us, (in the 7th Epistle of his first book), That, in a wound of the eye, it happened, through neglect, to unite and adhere to the under eye-lid; so that, after the cure, that eye was tied down, and rendered incapable of following the motions of the other: This occasioned every thing to appear double, till the eye, by its frequent motions, had at last stretched the eye-lid, to which it was adherent, and thereby recovered its former liberty of moving uniformly with the other.

Multitudes of cases of this kind might be advanced, but I like not, without necessity, to multiply examples of the same nature; these are sufficient to prove, that, when our eyes are restrained from moving uniformly, all objects are seen double. Neither is it to be doubted, but

but, when the same phenomenon occurs in drunk or maniac persons, it proceeds from the like cause: The uniform motion of our eyes requiring an easy and regular motion of the spirits, which frequently is wanting in such cases.

The same thing does also happen sometimes soon before death, when the spirits have been worn out and exhausted by long sickness. We have a remarkable example of this in the *Acta Hafniensia*, published by Bartholin. Olaus Borrichius, there tells us (vol. 2. p. 198.) of a woman that had been long ill of a disease in her breast and spleen, to whom, two days before her death, all things appeared double. He indeed attributes this phenomenon to a change in the figure of the humors of the eye, and thinks that they had acquired the form of a polygon, or multiplying glass; which is a very strange out of the way notion, and altogether improbable. The true cause thereof seems to have arisen from the languid irregular motion of the animal spirits disqualifying them from executing the commands of the will, and directing both eyes to the same object.

For these, and such like reasons, it is, that very many, both physicians and philosophers, have been brought to believe, that, to see objects single, it is absolutely necessary that both eyes be directed to the same object, and that this is one of the final causes of their uniform motion; and yet, when the matter shall be duly examined, I am confident, little foundation will be found for any such consequence. But

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I must delay entering upon this subject, till I have assigned what to me appears to be the true reason of this phænomenon; because a principle or two will be there established, necessary to be carried along in our thoughts throughout the whole of this argument.

Why objects seen with both eyes do not appear double, is a problem that has employed the genius and invention of the greatest men of all ages. Gassendus and Porta found such difficulty in reconciling this appearance with the ordinary principles of philosophy and optics, that they have been forced to suppose, that, though both eyes are open, yet we only see with one at a time. But this, being so obviously contrary to common experience, serves more as an example to shew what surprising lengths fancy may carry even the greatest men, than to satisfy the curious.

Galen imagined that this single appearance of objects proceeds from the close coalition of the optic nerves behind the *os sphenoides*, and seems to triumph in the discovery, as if he had found out a fine reason why our optic nerves are thus united, (*lib. 10. de offic. part. cap. 14.*) In this he is followed by a great many philosophers as well as physicians, though they are much divided among themselves in their manner of explaining it. Galen himself had recourse to a communication of pores, and followed the doctrine of Herophilus in giving to each nerve one pore, which he made to communicate at the conjunction of the nerves. But as this communication of pores is, by our best anatomists, now looked on as mere hypothesis,

thesis, that has no foundation in nature, so it is altogether insufficient for solving the problem, though, in place of one pore, each nerve should, agreeable to the more modern notion, be allowed to have as many of them as there are nervous fibres, as some of the followers of Galen have indeed supposed.

Our justly eminent Sir Isaac Newton (in the queries annexed to his optics) accounts for it thus: "Are not the species of objects seen
"with both eyes united, where the optic
"nerves meet before they come into the
"brain? the fibres on the right side of both
"nerves uniting there, and, after union, going
"thence into the brain, in the nerve which is
"on the right side of the head; and the fibres
"on the left side of both nerves uniting in
"the same place, and, after union, going
"into the brain, in the nerve which is on
"the left side of the head; and these two
"nerves meeting in the brain, in such a man-
"ner that their fibres make but one entire
"species or picture, half of which, on the
"right side of the sensorium, comes from
"the right side of both eyes, through the
"right side of both optic nerves, to the
"place where the nerves meet, and from
"thence on the right side of the head, into the
"brain; and the other half, on the left side of
"the sensorium, comes in like manner from
"the left side of both eyes."

This is indeed the most beautiful and ingenious explication of the manner how an object appears single from the coalition of the optic nerves that ever appeared; and to render

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der it still more probable, the same great man observes, "That the optic nerves of such animals as look the same way with both eyes (as of men, sheep, dogs, oxen, &c) meet before they come into the brain; but the optic nerves of such animals as do not look the same way with both eyes (as of fishes, and of the chameleon) do not meet." These reasons do indeed render his hypothesis very probable; yet there are others so demonstrative of the contrary, that it is not to be doubted, but, had they been known to Newton, he had retracted his opinion, especially since the thing admits of an easy solution, without any such supposition: For,

Although the optic nerves are united at the *sella turcica*, yet this happens without any confusion or decussation of their fibres. It is indeed true, that their conjunction is so close, that their substances seem to be confounded, yet there are several observations which prove that they are united only by a close conjunction, without any decussation, intersection, mixture, or confusion of substance, of which I shall only mention two; the one is from the accurate and faithful anatomist Vesalius. His words are: "Porro, iis qui de congressu hoc meatuque acriter citra partium inspectionem indies altercantur, non gravabor duo, quae in congressu animadverti, hic adjicere, quo et hinc suarum nugarum habeant argumenta. Patavii itaque adolescens suspendio necatus, publicae sectioni adhibitus fuit, cui ante annum dexter oculus a carnifice erutus fuerat: Deinde mulier, eodem supplicio affecta, nobis obtigit, cui dexter quoque oculus

" ab ineunte aetate emarcuerat, sinistro interim
 " integerrimo. Mulieri dexter nervus toto pro-
 " gressu longe tenuior sinistro visebatur, non so-
 " lum extra calvariae cavitatem, verum in exortu
 " quoque, et in dextra congressus nervorum se-
 " de. Ac praeterquam quod dexter tenuis e-
 " rat, durior quoque et rubicundior cernebatur,
 " uti sane et in adolescente ; sed dexter non ad-
 " modum, neque crassitie, neque mollitie adhuc
 " sinistro cedebat." (*De corp. hum. fabrica*,
 lib. iv. c. 4.) The other observation which I
 shall take notice of, is from Cæsalpinus, whose
 words, as recorded by Riolan, (*Anthropograph.*
 lib. iv. cap. 2.) and Diemerbroeck (*Anatom.*
corp. human. lib. iii. cap. 8.) are as follows,
 " Repertus est aliquando in anatome alter ex
 " nervis visoribus attenuatus, alter plenus : Visus
 " autem erat imbecillis in oculo ad quem ner-
 " vus extenuatus ferebatur ; habuit enim vul-
 " nus in capite circa eandem partem ; nervus
 " autem extenuatus non ad oppositam partem
 " procedebat, sed ad eandem flectebatur. Visum
 " hoc est Pisis anno 1590. Unde omnes spe-
 " ctatores argumentum id certum existimave-
 " runt, nervos visorios nequaquam se interfecare,
 " sed coire et regredi ad eandem partem."

From these and such like observations, it
 plainly follows, That our optic nerves do not
 intersect each other, nor mix and confound their
 substances, but are only united by a close co-
 hesion, contrary to what the opinion of New-
 ton and the Galenists supposes.

But this is not all ; for, supposing our optic
 nerves to be united in what manner they please ;
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with both eyes, does not depend on this union, seems evident from another observation of the same Vesalius, which being a little uncommon, I shall also set down at length in the author's own words. "His ille accessit" (says he, speaking of the former observations) "cujus nervos visorios illo de quo hic sermo est congressu invicem non connasci, neque sese contingere, vidimus; sed dexter nonnihil sâ sede, quâ calvariam egressurus fuerat, sinistrorsum, et sinister nonnihil dextrorsum, reflectebatur, quasi non coalitûs occasione nervi congregerentur, verum ut commodè per suum foramen è calvaria prociderent; potissimum quum etiam hoc ductu progredientes, in oculi posterioris sedis medium non inferantur. Quam sedulo autem ac sollicitè ejus viri, cui in eum modum nervi dehiscebant, familiares, num illi omnia gemina perpetuo oculis obversarentur, interrogaverimus, neminem naturæ operum cognitione flagrantem ambigere fatiscio; at nihil aliud resciscere licuit, quam ipsum de visu nunquam conquestum fuisse. visque præstante semper valuisse, familiaresque de visorum duplicatione nihil unquam intellixisse." Seeing then that objects have appeared single to such as had their optic nerves disjoined, it must be allowed that this phænomenon depends on something else than the coalescence or decussation of these nerves. Others therefore, laying aside all the hypotheses that are founded upon any coalescence, contact, or crossing of the optic nerves, have sought for the cause of this phænomenon in a certain sympathy betwixt them. To explain this sympathy, Mr Rohault (*Physic. part. 1. cap. 31.*) supposes, that in each nerve

there are just as many fibres as in the other, and that the corresponding fibres of both nerves are united in the same point in the sensorium, *e.g.* Suppose, as in Tab. I. Fig. 1. the nerves composed of five fibres, whose extremities in the right eye are A, B, C, D, E, and in the other eye, *a, b, c, d, e.* The corresponding fibres, Aa, Bb, Cc, Dd, and Ee, are supposed to meet in the sensorium S, in the points *a, b, c, d, e.* Hence, if both eyes are directed to F, its image will fall on the retina at the optic axes, and there strike the sympathizing fibres C and *c*; which motion being propagated to the single point of the sensorium *c*, must there make but one species or picture. In like manner, the eyes retaining the same direction, the image of the point G will fall upon the right side of both eyes; and, by striking the correspondent fibres E and *e*, will, in the sensorium, make but one impression at *e*, where these fibres terminate; and the image of the point H, by striking the corresponding fibres A and *a*, will, in the sensorium, make but one impression at *a*: And thus, though both eyes receive the same impressions from objects, yet they are not seen double, because, of these two impressions or images, one is only formed in the sensorium.

But neither has this hypothesis, however specious, any foundation in nature, for if, with Des Cartes, we suppose the *glandula pinealis* to be the sensorium, or chief seat of the soul, anatomy teaches us, that the nerves are not inserted into it; and if, with Willis, we should place the sensorium in the *corpora striata*, or any other part of the brain, these being double

and alike on both sides, can never make one individual sensorium, in which all the corresponding analogous nervous fibres are united.

The judicious Dr Briggs (as may be seen at large in the Philosophical Transactions, and in his *nova visionis theoria*, annexed to his ophthalmographia) has invented another hypothesis for explaining this supposed sympathy of our eyes, without having recourse to any meeting, communication, or conjunction of the nervous fibres in the sensorium. He supposes that the optic nerves consist of homologous fibres, which have their rise in the *thalami nervorum optitorum*, and are thence continued to both retinae, and that these fibres have the same situation, disposition, and tension in both eyes, *e. g.* he supposes that the fibres going to the upper part of the retinae have a greater degree of tension; those going to the under part, a smaller degree of tension; and those going to the corresponding sides, corresponding degrees of tension, and so forth; and consequently, says he, when an image is painted on the corresponding parts of each retina, the same effects are produced, the same notice or information is carried to the thalamus, and so imparted to the soul, or judging faculty; for the homologous and corresponding fibres of both retinae, upon which the image falls, having the same degree of tension, may be conceived as cords of two musical instruments in concord and unison, which, from the impressions of light, are put into the same vibrations; so that the mind can have but one sensation from the same object, since

since the two impressions are reunited in one, by the similar and like disposition of the fibres of the two nerves, which do so correspond with each other, and which have such a conformity and similitude in their vibrations, that the soul cannot hinder itself from identifying the two impressions which it receives therefrom.

This is the substance of Dr Brigg's hypothesis; to confirm which, he flies to experience and observation, pretending that this variety of tension in our nervous fibres, is owing to their greater or lesser flexure in the thalami, which, he says, is manifest to the naked eye; and finding that the fibres on the inside of both thalami agree in flexure, as also those on the outside, &c. he concludes that they agree also in tension, and consequently sympathize by a similitude in their vibrations. But, unluckily for the author, this curious observation of his is so far from confirming his hypothesis, that it quite undermines it; for all objects, a little to a side of the concourse of the optic axes, would then appear double, by being painted on dissimilar parts of the retina; whereas, had he laid aside his observation, and with Newton, Rohault, &c. contented himself in supposing, that the fibres on the inside of one eye sympathize with the external fibres of the other, his hypothesis had not been chargeable with this absurdity, though even then many reasons are not wanting for rejecting it: As,

1. It is a very difficult matter to conceive how the soft, tender, and delicate fibres of the retina

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retina and medullary part of the optic nerves, can, without breaking, suffer that strong tension which seems necessary to qualify them for being put into those vibrating motions, in which he makes vision to consist. And it is more probable, that the impressions made upon our organs produce an undulation and refluatation of the spirits, or of Newton's *materia subtilis* in the nervous fibrils, which reaching the sensorium, gives us the ideas of objects, than that these ideas should be excited by these vibrations themselves. This I might easily evince from a great many arguments, which I must not now mention, because they would carry us too far out of our road: And especially, because, though we allow all sensation to proceed from the vibrations of our nervous fibres, yet this does not appear sufficient to establish his hypothesis: For,

2. Supposing all sensation to proceed from vibrations excited in the nervous fibres, and that these fibres, in the analogous and corresponding parts of the retinae, have the same degree of tension: *ex. gr.* Suppose the tension of the superior fibres of both retinae to be the same, as also that of the inferior; but that the fibres which terminate in the superior part are more tense than those that go to its inferior part: This is what our author supposes. Now, according to this hypothesis, it would follow, that vision would be more clear and strong when caused by rays striking the upper part of the retina, than when caused by rays striking its inferior part, where the fibres being less stretched, must make the vibrations more languid and faint;

faint; and consequently an object placed below the optic axis, by painting its image upon the superior part of the retina, would appear more clear and lively than when it is placed above it: But this is contrary to all our experience; and what we have said of the superior and inferior fibres, holds equally in all the rest: For vision being every where uniform, it is necessary that the fibres, by whose vibrations it is supposed to be occasioned, should be every where equally stretched.

3. If the concord, harmony, and equal tension of the analogous and corresponding nervous fibres, were the reason why objects seen with both eyes appear not double, it is not easy to conjecture, why this depravation of sight does not happen oftner that it is found to do in practice, and especially in such as abound with ferous and phlegmatic humors, and whose brain is moist and lax, and who thereby are disposed to the *gutta serena*, or subject to convulsions and other nervous diseases; for, in such, the fibres of one of the optic nerves may easily happen to be relaxed, while the corresponding fibres of the other nerve still retain their natural tension; and, in particular, why did not that woman, formerly mentioned from Caesalpinus, see objects double, since one of the optic nerves was found withered and emaciated, while the other remained moist, plump, and juicy as before? Surely one would expect that this could not have happened in the nerves, without affecting the tension of their fibres, and thereby have occasioned objects to appear double; yet no such thing happened;

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happened; for every thing appeared single as before, only the sight of the diseased eye was more faint and obscure than in the other: From which it follows, that the cause of this phænomenon is to be sought for some where else, than in the similar situation, disposition, and tension of the nervous fibrillæ.

The true cause why objects appear not double, though seen with both eyes, to me seems wholly to depend on the faculty we have of seeing things in the place where they are. But, to explain and confirm this, I must premise the following

L E M M A.

“Every point of an object appears and is seen
“without the eye nearly in a straight line,
“drawn perpendicular to the retina, from
“that point of it where its image falls.”

That this is true, we may gather from many experiments. The following is very easy and convincing.

Suppose *E* the globe of the eye, furnished with all its coats and humors (See fig. 2. and 3.) and let *A* be a small object, such as the head of a pin, whose distance from the eye must be greater or less than that at which an object would be most distinctly seen when viewed with the naked eye. Close to the eye place a card, or piece of opaque paper, in which is a small hole made with a pin, and let *QT* represent the card. If this hole be supposed at *x*, then the ray of light *Axn*, falling on the retina at *n*, will there paint the image of the small object *A*, and make it appear

pear in the visual line nxA , which is perpendicular to the retina at the point n . But, if the card be brought lower, so as its small hole may be at r , the ray of light Ar , after passing the hole, will be refracted in the eye, so as to fall upon the retina at some other point, as m : For the object A being supposed at a greater or lesser distance than that at which an object may be most distinctly seen with the naked eye, all its rays that pass the pupil must be made to converge to a point, either before or behind the retina, such as o ; but, on the retina itself they must fall on different points, according to the situation of the hole through which they pass; for the eye does not change its conformation, and adapt itself to the distance of an object viewed through a perforated card, as it always does when objects are viewed naked without any such interposition. Now the object A seen through the hole r , does not appear in its real place A , but at some other place, as B , viz. in the right line mB , which is perpendicular to the retina at the point m ; and if the card be raised, so as the ray Ad may pass the hole at d ; after refraction, it will fall upon the retina at i , and the object will appear nearly at C in the perpendicular iC .

In like manner, if the card be pierced with three small holes, whose distance does not exceed the diameter of the pupil, as in d , x , and r , then the little object A will, at the same time, be seen in three different places, C , A , and B , and must therefore appear multiplied, according the number of holes; which evidently

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vidently proves, that the rays that flow from the object through these holes, fall upon different points of the retina; and that there are three lines drawn perpendicularly from these points in the retina, in each of which the object is distinctly seen. We might here observe, that if the object A be brought to that precise distance from the eye that is necessary for uniting all its rays in one single point of the retina, as n , then it will appear single, though viewed through several holes: And the same thing must happen, though the middle hole be covered, so as no rays fall upon the eye but what pass at the holes d and r , towards the extremities of the pupil; for, these rays being united in the retina at n , the object will be seen in the visual line nxA , tho' no rays pass that way. From all which it is evident, that every little object, or point of an object, appears and is seen in the visual line, drawn perpendicular to the retina, at that point of it where its image falls.

To make this experiment with exactness, you must, for an object, look to a small luminous point in a dark place, such as a little hole in a card placed before a candle, or else you must look to a small black object placed on a white or luminous surface, as has been observed by Sche-
neir, (*Fundament. optic.*) who has several other curious enough observations relative to this experiment. Any trials I had occasion to make succeed well enough with a pin placed before a well-lighted window.

It is from this principle, that, when a man in the dark rubs the under part of either cor-

ner of his eye with his finger, and turns his eye away from his finger, he will see, towards the opposite side of the eye, a circle of firey colours like those in the feather of a peacock's tail, which can arise from nothing but such motions excited in the retina by the pressure and motion of the finger, as at other times are excited there by light coming from the opposite side for causing vision. Whence this experiment may be of use in distinguishing a cataract that admits of the operation from one that is complicated with an amaurosis, or paralyfis and insensibility of the nerve: For if, in rubbing the eye in the manner just now mentioned, no coloured circle appears, it is a sign that the eye is paralytic, and that nothing is to be expected from the depression of the cataract: Whereas, on the contrary, the appearance of such a circle plainly shews that the nerve is not affected, and consequently that the success of the operation is not to be wholly despaired of, even though the pupil should be without motion: For such an immobility of the pupil does not always proceed from an insensibility of the nerve, but is also sometimes occasioned either by the pressure of the cataract against the uvea, or by their adhesion; neither of which are esteemed sufficient to render the operation always fruitless.

It is likewise from this principle, that an object seen through a prism appears so far removed from its true place, and that an object seen through a polyedrum or multiplying glafs, ap-
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appears in so many places at once, and therefore appears as often multiplied.

The same principle is also confirmed from the erect and natural appearance of objects, though their image on the retina be inverted: Thus (in fig. 1) H , the lower part of the object GH is projected on a , the upper part of the eye, and the highest part G is projected on the lowest part of the eye, which makes the image or representation ae inverted; yet, because the point G appears without the eye in the line eG , and the point H in the line aH , the point G must of necessity be seen higher than the point H .

What hath occasioned some seeming difficulty in the business of erect appearances, is the groundless supposition that the eye, or rather the soul, by means thereof, sees an inverted image of the external object painted on the retina, and that it judges of the object from what it observes in this image: But this is a vulgar error, and I appeal to any one's experience, whether he ever sees any such thing, and every one is himself best judge of what he sees; and, as the mind sees not any image on the retina, so it takes no notice of the internal posture of the retina, or the other parts of the eye, but useth them as an instrument only for the exercise of the faculty of seeing; and therefore, when the retina on its lower part, at the point e , receives an impulse or stroke from the rays that come from the upper part of the object G , is it not more natural, as well as useful, that the mind, without any regard to the situation of that part of

the retina, should, agreeable to the principle here laid down, be directed to consider the stroke as coming from the upper part of the object G, rather than from its lower part H, and consequently to conclude the cause of it, or the object itself there also? And what is said of upper and lower, holds equally in sinister, dexter, and all other parts of the object.

This may be illustrated, by conceiving a blind man, who, holding in his hands two sticks that cross each other, doth with them touch the extremities of an object, placed in a perpendicular situation: It is certain this man will judge that to be the upper part of the object which he touches with the stick held in the undermost hand, and that to be the lower part of the object which he touches with the stick in his uppermost hand.

SCHOLION.

“ The judgment we form of objects being placed without the eye in those perpendicular lines, or, which is nearly the same thing, the judgments we form of the situation and distance of visual objects, depends not on custom and experience, but on an original connate and immutable law, to which our minds have been subjected from the time they were first united to our bodies.”

That the truth of this may appear, it may be requisite we look a little into the nature of our sensations, and carefully observe what is meant in common discourse, when one says he sees an object.

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Every body knows, that, properly speaking, colours are the only proper objects of sight. Now colours may be considered five ways.

1. They may be considered as the properties inherent in the light itself. What these properties are, was in the year 1666 first discovered by the incomparable Newton, and afterwards published in the Philosophical Transactions, where he also gave a specimen of the experiments he made for confirming his doctrine. After that, in the year 1704, the same great man proposed the same doctrine more fully, in his beautiful Treatise of Optics, and confirmed it with great variety of convincing experiments; where he has demonstrated, that light is not all similar and homogenous, but compounded of heterogenous and dissimilar rays, each of which are endued with different properties; some, at like incidence, being more refrangible, and others less refrangible; and those that are most refrangible, are also most reflexible: And, according as they differ in refrangibility, or reflexivity, they are endued with a power of exciting different motions or agitations in our retina; which, being propagated through the nerves to the sensorium, give us those different ideas which we call colours. So that colours, considered as properties of light, are not modes or qualifications of light derived from the various terminations of light and shadow, or from refractions or reflexions of natural bodies, as was the general opinion of former philosophers, but original, connate, and immutable properties, which in divers rays are divers: Some

rays, when alone, being of such magnitude, figure, or solidity, as disposes them, by striking the retina, to exhibit a red colour and no other; some an orange and no other, and some a yellow and no other; and so of the rest of simple colours, green, blue, indigo, and violet. But, when they are mixed and blended promiscuously, they exhibit compound colours of different sorts, according as the light is composed of more or fewer of the different-coloured rays, or as they are mixed in various proportions.

2. Colours may be considered as qualities residing in the body that is said to be of such or such a colour; and, in this sense, colours are nothing but the various dispositions of the surfaces of objects, whereby they are qualified to reflect only the rays of one sort of colour, or at least in greater plenty than the other colours. This was experimented in a dark room, by illuminating objects successively with uncompounded light of divers colours; for by that means any body may be made to appear of any colour. They have there no appropriate colour, but ever appear of the colour of the light cast upon them; but yet with this difference, that they are most brisk and vivid in the light of their own day-light colour. Minium appeareth there of any colour indifferently, with which it is illustrated, but yet most luminous in red; and so bise appeareth indifferently of any colour with which it is illustrated, but yet most luminous in blue; and therefore minium reflecteth rays of any colour, but most copiously those endued with red;

red; and consequently, when illustrated with day-light, that is, with all sorts of rays promiscuously blended, those qualified with red shall abound most in the reflected light, and, by their prevalence, cause it to appear of that colour. And, for the same reason, bise reflecting blue most copiously, shall appear blue, by the excess of these rays in its reflected light, and the like of other bodies. And that this is the entire and adequate cause of their colours, is manifest; because they have no power to, change or alter the colours of any sort of rays incident apart, but put on all colours indifferently with which they are enlightened. But we must remit those who desire full satisfaction in this and the foregoing point, to that admirable treatise of optics written by Newton; for it is impossible to separate the parts of this work from one another, without disadvantage to them, or to sum them up in less room, without losing some things very useful and beautiful; that great person, having before shewn how far numbers and geometry would go in natural philosophy, has, in his optics, manifested to the world to what surprising height even vulgar experiments, duly managed and carefully examined in such hands, may advance it.

3. Colours may be conceived as the passion of our organ of sight, that is, the agitation of the fibres of the retina by the impulse or stroke received from the rays of light; which agitation is communicated to the sensorium, or that part of our brain in which our mind
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does principally reside, else it could perceive nothing.

4. Colours may be considered as the passion, sensation, or perception of the mind itself, or that which all of us perceive, when we look at any object. It is this only that, properly speaking, deserves the name of *colour*; for colours in objects are nothing but their disposition to reflect this or that sort of rays more copiously than the rest, and in the rays of light they are their disposition to excite this or that motion in the organs of vision, and in them they are only different undulations in the animal spirits, which are propagated through the nervous fibres to the sensorium. In all this there is no perception, no sensation, no colour; for, to speak truly, colours are sensations produced in our mind, and do not belong either to light, objects, our organs, or to any other body, but are the modifications of the mind itself, which always stand connected to what passes in the organs and sensorium.

5. The *fifth* manner in which colours may be considered, is the judgment which our mind naturally forms, when it concludes, that that which it feels or perceives is in the body itself said to be coloured, and not in the mind. How body acts upon mind, or mind upon body, I know not; but this I am very certain of, that nothing can act, or be acted upon where it is not; and therefore our mind can never perceive any thing but its own proper modifications, and the various states and conditions of the sensorium to which it is present: For, when

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when I look at the sun or moon, it is impossible that these bodies, so far distant from my mind, can, with any propriety of speech, be said to act upon it. To imagine otherwise, is to imagine things can act where they are not present; which is as absurd, as to suppose that they can be where they are not. *Nam virtus sine substantia subsistere non potest. Newton princip. mathemat. schol. general. sub fin.* These bodies do indeed emit light which, falling upon the retina, does excite certain agitations in the sensorium, and it is these agitations alone which can any way act upon the mind, and therein excite those modifications which we call colours; so that it is not the external sun or moon which is in the heavens which our mind perceives, but only their image or representation impressed upon the sensorium. How the soul of a seeing man sees these images, or how it receives those ideas from such agitations in the sensorium, I know not; but I am sure it can never perceive the external bodies themselves to which it is not present: So that, properly speaking, the colours which our mind perceives, when we look at any object, are only the modifications of the mind itself, arising from the motions excited in the sensorium; and, when we imagine we see these colours in the external object itself, this certainly is not a perception, but a judgment or conclusion, whereby we attribute that which our mind feels to external objects.

What we have said with respect to colours, may, without difficulty, be also applied to our other

other sensations: For we not only ascribe the colours we perceive to the objects we look at, but also we judge that our other sensations are in the other objects of sense; thus, when I taste sugar, I conclude it is sweet; and when I smell camphire, or such like odoriferous bodies, I am naturally inclined to look on that smell, which I perceive, as a quality inherent in the object; and yet it is certain that these different sensations are only the modifications of the mind itself, and do not belong to the objects to which they are generally attributed.

It is indeed true, that our mind does not always attribute its own proper sensations to the external objects that produce them; for sometimes it ascribes them to the organs, at other times both to our organs and the object.

That we may explain how this happens, we must consider that the sensations of our mind are of three different sorts; some are very strong and lively, others weak and faint, and a third sort are of a middle nature betwixt both.

Our strong and lively sensations are those that touch our mind very sensibly, and, as it were, surprise it and rouse it up with force and vigour; and of this sort are all those sensations which are very agreeable or painful, such as that which arises when one is tickled with a feather or burnt with the fire, and in general when the organs suffer any thing that is capable of hurting or destroying the body; in all these cases our sensations are so brisk and lively, that the mind can scarce hinder itself from looking on them as in some sort belonging to itself;

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itself; and therefore it does not judge that they are in the objects, but believes them to be in the members of its body, which, because of the strict union betwixt mind and body, it considers as a part of itself. Thus, when my hand is pricked with a pin, burnt, or otherwise hurt, I am naturally directed to attribute that painful sensation to that part of my hand on which the impression is made, and not to the fire, pin, or other object producing it, and yet it is certain, that pain is a sensation or modification of the mind itself, and belongs as little to our organs as colours do to objects.

The *second* sort of sensations are the weak and languishing, in which the mind is but very slightly touched, and which are neither very agreeable nor disagreeable, as light, when not too strong; all manner of colours, tastes, and smells, moderate sounds, &c. These sensations do so slightly affect our mind, that it never thinks that they belong to it, nor that they are in the body to which it is united, but only in the external objects that produce them. It is for this reason we rob the mind of its own proper sensations of light and colour, therewith to deck and imbellish the object; and yet there is nothing more evident, than that all of them are the modifications of the mind itself, and noways inherent in the objects, in which our mind places them.

The *last* sort of our sensations are neither strong nor faint, but of a middle nature betwixt both, such as heat and cold when moderate, great light, violent sounds, &c. And here it may
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be observed, that a weak and languishing sensation may become both a middle or strong one; as for instance, the sensation of light is weak, when the light of a candle or flambeau is faint, or when it is at any considerable distance; but, this sensation may become a middle one, if the flambeau be brought near enough the eye for to dazzle; and likewise it may become very strong and vivid, provided that it be brought so near as to burn. Thus the sensation of light may be weak, strong, or betwixt both, according to its different degrees. Now these middle sensations do neither touch the mind very sensibly, nor very slightly; hence it is that it is very much embarrassed and straitned where to place its sensation; for, upon the one hand, it is inclined to follow the natural judgment of the senses, in removing from itself, as much as possible, these sorts of sensations, to attribute them to the external objects; but, upon the other hand, it cannot altogether hinder itself from looking on them as in some sort belonging to itself, especially if they approach to those which we have called strong and brisk: And this is the reason that the mind judges that cold, heat, and the other middle sensations are not only in the ice, fire, and other objects producing them, but also in that part of the body itself, upon which the impressions are made.

Thus you see how the mind never considers its sensations as belonging to itself, but always attributes them either to the object, our organs, or both, according as they are of a sluggish, brisk, or middle nature; and though

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it is certain that they are the modifications of the mind itself, yet since our senses are not given us to inform us what things are in themselves, but only what they are relatively to our bodies, it was very necessary that they should incline us to judge of sensible qualities in the manner they do: As, for instance, it is much more profitable for us to feel pain and heat, as in our body, than that we should judge them only in the objects that caused them; for, since they are capable of hurting our members, it was necessary that we should be advertised when they are thereby attacked, in order to secure ourselves therefrom: But it is not so with respect to colours; for they do not ordinarily hurt the retina on which they fall: And it is altogether useless for us to know that they are painted there. Colours are not necessary, unless to know objects more distinctly, and at a distance; and it is for this reason that our sight ever inclines us to attribute them to objects. From all which it is manifest, that the judgements which our senses induce us to make concerning the sensible qualities, are exceeding just, if considered with respect to the preservation of our bodies, for which they were only given us; though, at the same time, it is most certain, that they are altogether extravagant, and vastly removed from truth.

From what has been said concerning the nature of our sensations, it is evident, that the mind never considers any of them as belonging to itself, but as belonging to something external. Now, since there is no essential or necessary connection betwixt these perceptions;

and the judgements we form concerning them, it follows that these judgments must either depend upon custom and experience, or on an original, connate, and immutable law.

That all of them should depend on custom and experience, is a contradiction in terms, it being impossible for us to have any experience, till some how or other we have formed a judgement; which judgement must therefore depend on an original, connate, and immutable law, that cannot but obtain at least in some of our sensations. To say otherwise, is to say something very absurd: It is to say we judge by experience that has never been experienced. Hence it is plain, that, when one says the mind by custom and experience comes to conclude what it sees to be without the eye in such perpendicular lines; this experience cannot be meant of sight, but of some of our other senses, such as that of feeling or touch; which therefore, by virtue of a connate and immutable law, must naturally, and of itself, without any assistance from the other senses, form a judgement concerning its own perceptions, and conclude, that they are not in the mind but in something external.

But, if by the touch alone we can judge thus of the situation and distance of external things, I see not why the same power should be denied to the sight. It cannot be said that it is more difficult for the mind to trace back the perceptions it has by sight, from the sensorium to the retina, and from thence along those perpendicular lines, to the object itself, than it is to trace back the perception it has by touch

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AND OBSERVATIONS. 183.

from the sensorium along the nerves to the external object occasioning them. The subjecting our minds to a law in seeing, is as easy as subjecting them to a law in feeling; and that, in feeling, our mind must be subjected to a law, directing it to consider its own sensations, as belonging to external objects, has been just now shewn; and this shews the tenor and course of nature, and makes it highly probable that the same thing obtains in our other senses; for nature is very consonant and conformable to herself, and very simple and uniform. She never performs similar and like effects in divers manners, and by dissimilar and unlike means; this were to destroy that simplicity and uniformity, which is the beauty of all her works, and which she is observed to delight so much in.

Were not the mind, in seeing, subjected to a law, whereby it traces back its own sensations from the sensorium to the retina, and from thence along the perpendicular lines above-named to the object itself, and thence concludes what it perceives to be in the external object, and not in the mind, it is plain, that a man born blind, being made to see, would at first have no idea of distance or situation by sight. The sun and stars, the remotest objects, as well as the nearer, would seem to be in his eye, or rather in his mind; and, if so, whence is it he comes afterwards to judge what he sees to be in the external object? This cannot proceed from experience alone; for, though by the touch we have frequently experienced the existence, distance, and situation

tion of things external, and found these ideas to have been preceded by certain corresponding visible ideas, I see not how, upon perceiving any visible idea present with our mind, we should judge it to be without in the external object, without subjecting our mind to an arbitrary irresistible law directing it so to do. This were to establish an essential and necessary connection betwixt these judgements, and the experiences we have by touch laid up in our memories; whereas it is plain no such thing can be, all the connection that is being only customary and experimental: And, seeing nature at any rate must be at the charge of a law, is it not more reasonable to suppose, that, by the sight alone, without any assistance from the other senses, the mind, in consequence of such a connate and immutable law, as has been allowed it in the judgements it forms by touch, should be enabled to trace back its own perceptions in the so often named perpendicular lines, to the object itself, and thence to form a judgement of its distance and situation? I say, is not this more reasonable, than to suppose that we stand in need of the experiences of touch? Could these experiences be of any use without a new law, there might be some pretence for such a supposition; but, this being impossible, it follows that the judgements we form of the situation and distance of visual objects, depend not on custom and experience, but on an original, connate, and immutable law, to which our minds have been subjected from the time they were first united to our bodies. To say otherwise, is to say that

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our sight is less perfect than our touch; not from any necessity in the thing itself, but from an error or mistake in nature. It is to make nature do something in vain, and to be luxuriant in superfluous causes, which is to break down the catholic and fundamental rules of philosophizing, established by Newton in his *principia philosophiæ*. It is to maintain, that one accustomed from the birth to see objects through a prism or polyedron, and not otherwise, would see objects as other men do, without any such interposition, which to me seems very absurd and ridiculous.

From what has been said, I think the truth of our scholium is sufficiently evident; but, because Dean Berkeley, in his *Theory of Vision*, has a good deal of subtile reasoning in favour of custom and experience, that has not yet been taken notice of, I shall stop a little to consider it.

He observes that, in seeing, the mind, strictly speaking, perceives nothing but what is present with it; and thence concludes, that the ideas of space, outness, distance, and of the situation and magnitude of things placed at a distance, cannot by the sight alone be introduced into our minds; but that having of a long time experienced certain ideas perceivable by touch, to have been connected with certain ideas of sight, we do, upon perceiving these ideas of sight, forthwith conclude what tangible ideas are, by the wonted ordinary course of nature, like to follow: And thus, by a habitual or customary connection that has grown up between these two sorts of ideas, the latter is always suggested by the former; just as things

are suggested by words, and shame by blushing: So that, according to him, when a man is said to see the magnitude and situation of this or that distant object, nothing is meant, but that the ideas of sight which are present with the mind, suggest to his understanding, that, after having passed a certain distance in such a direction, to be determined by the motion of his body, which is perceivable by touch, he shall come to perceive certain tangible ideas, which have been usually connected with them; but, because there is no essential or necessary connection between the ideas of sight and touch, the ideas, suggested by sight, of the distance, situation, and magnitude, of external things, must depend entirely on custom and experience; for, that one idea may suggest another to the mind, it is sufficient that they have been observed to go together, without any demonstration of the necessity of their co-existence, or without so much as knowing what it is that makes them so to co-exist.: Thus words signify things, for no other reason, than barely because they have been observed to accompany them.

In answer to this, I must ask Mr Berkeley, how it appears that the ideas we have by sight of the distance, situation, and magnitude, of external things, are nothing but the tactile ideas suggested to our minds? When my eyes are shut, I can at pleasure recal to mind the ideas of touch, which former sensations had lodged in my memory, the bare naming the thing doth presently suggest them as well as the seeing it: But there is no body who doth

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not perceive the difference in himself between actually looking upon an object, and contemplating the idea he has of it in his memory; and therefore he hath certain knowledge they are not both memory or fancy. But this is not all: For,

Though it must be acknowledged, that, in seeing the objects, the mind, strictly speaking, perceives nothing but what is present with it; yet it does not from thence follow, that the ideas of space, outness, distance, and of the situation and magnitude of things placed at a distance, cannot, by the sight alone, be introduced into our minds. Were this true, I see not how we could ever come by those ideas; for the tangible ideas are as much present with the mind as the visible ideas, and on that account must be equally incapable of introducing the idea of any thing external. When with my hand I touch an object, the idea perceived is present with my mind; and, in moving my hand along the object, or in moving my body from one place to another, the ideas or perceptions that succeed one another are all of them as much present with my mind as any visible idea can be. How then can it be said, that external distance and situation are only determined by the motion of the body, perceivable by touch? This is to destroy the universally received notions we have of things, and to confound external space, distance, and situation, with a series of ideas succeeding one another in the mind. It is to take away all difference between space and time, and to make both consist in a consciousness of a succession

cession of different ideas or perceptions in the mind; whereas it is certain, that neither of them depends on our ideas, but must continue the same whether we have any ideas or not. It is to introduce a wild and unbounded scepticism, a scepticism that at once banishes this external world, and space itself, out of the field of existence, and in place thereof substitutes a visionary world of ideas and phantoms, existing no where but in his own mind. It is to suppose that it is GOD who presents the appearances of things external to us, and that he does it in such a manner as to deceive us: For, supposing things external to exist, I see not what greater assurances we could have of their existence than we now have. In fine, this is not to solve the problem, whether it be from custom and experience, or by virtue of an original connate law, that by sight we come to judge of the situation of external things, but, by exterminating all things external, to make the problem itself absurd and ridiculous.

If what has been demonstrated in the preceeding lemma be duly attended to, it will not be difficult to explain how objects seen with both eyes appear single, from their being seen by each of them in the same place.

For illustrating this, let it be supposed, that both eyes are directed to the point F, (See Fig. 1.), by the preceeding lemma, this point must be seen by the left eye in its axis cF , and by the right eye in its axis CF : And, since we have also a faculty of judging of the distance of objects, it follows that the point F must be seen

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seen by both eyes in that precise place where the lines cF , CF intersect each other; and, being seen in the same place by both eyes, it must necessarily appear single, it being impossible for us to conceive two objects existing in the same place at the same time.

In like manner, all the other visual points of the object GH must also appear single; for, supposing the eyes to continue their former direction, the rays, which come from any other point, as H , will be united on the retina of the left eye on the outside of the optic axis at a , and in the other eye they will be united on the inside of its axis at A : Hence the point H will be seen by both eyes in the lines aH , AH , which are perpendicular to the retina, at the points a and A , where the image is painted; but the lines aH and AH meet, and intersect each other at H : And therefore, since our mind or visive faculty has a power of judging rightly of the distance of objects, it follows, that the point H must be seen by both eyes in the precise point H , where these lines intersect each other; and consequently must appear single, because we can have no idea of the penetration of matter; or, which is the same thing, it is impossible for us to conceive two visible objects placed in the same place at the same time.

What hath been just now said of the single appearance of objects seen with both eyes, holds only with respect to objects placed in the plan of the horopter; for all objects placed out of this plan, must, in consequence of the principle laid down in the foregoing lemma, be seen
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in two different places, with relation to that point of the horopter on which our eyes are fixed; and, being seen in two different places, must therefore appear double.

Thus, if, while the optic axes, (See Fig. 4.) AC, BC, are directed to a mark C, for viewing it accurately, we attend to an object x , placed any where within the angle ACB formed of the optic axes, the object x will appear in two places; for, being seen by the right eye in the direction of the visual line Bx, it must appear on the left side of C, and its distance from C will be measured by the angle CBx; and, being seen by the left eye in the direction of the visual line Ax, it must appear on the right side of C, and its distance from C will be measured by the angle CAx; and consequently it must appear double, and the distance between the places of its appearance will be measured by the sum of the angles CBx, CAx. For the like reason, so soon as the eyes change their direction from C to x , the object x shall appear single, but all other objects placed within the angle DxE made by the optic axes produced will appear double. Thus, the object C, being seen in the visual lines AC, BC, which are on different sides of the object x , it must necessarily appear double, and the distance between the places of its appearance is measured by the sum of the angles CAE, CBD.

The same way of reasoning, applied to objects in all manner of situations, will shew, that all of them must appear double, when placed out of the plan of the horopter; all which is exactly

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exactly agreeable to experience: And this also is the reason why a double appearance will be seen when the end of a long ruler is placed between the eye-brows, and extended directly forward with its flat sides respecting right and left; for, by directing the eyes to a remote object, the right side of the ruler seen by the right eye, will appear on the left hand, and the left side on the right hand. But we are not from this to imagine, that the single appearance of objects placed in the plan of the horopter, arises from the uniform motion of our eyes; for, while the left eye is directed to F (See Fig. 1.) let the other be directed to G or H, it is plain, from the preceeding lemma, that the points G, F, and H, will continue to be seen in the same perpendicular lines EG, CF, AH, they formerly appeared in, when both eyes were directed to F; and, since at the same time we have a power of judging rightly of their distance, it follows, that here also they must appear single, from their being seen in the same place by both eyes.

How this agrees with the double appearance of objects, when either of the eyes is, by the pressure of the finger, or by a spasm or paralysis in any of its muscles, restrained from following the motions of the other, will be shewn hereafter. In the mean time, from what has been already said, we may see the reason why those who, from use and custom, have acquired a power and habit of squinting, see objects single as other men.

But, for the better explaining the nature of squinting, and, from the above established principles,

ciples, to set the diagnostics and prognostics of this disease in a juster light than hitherto has been done, it will be necessary to take a view of the several causes from which it may proceed, and to determine some of the chief optical effects that naturally arise from them, whereby the preceding doctrine will be further illustrated and confirmed. And,

First, This disease may proceed from custom and habit, while, in the eye itself, or in its muscles, nothing is preternatural or defective: Thus children, by imitating those that squint, and infants, by having many agreeable objects presented to them at once, which invite them to turn one eye to one, and the other eye to another, do frequently contract a habit of moving their eyes differently, which afterwards they cannot so easily correct. Infants likewise get a custom of squinting, by being placed obliquely towards a candle, window, or any other agreeable object capable of attracting their sight; for though, to see the object, they may at first turn both eyes towards it, yet, because such an oblique situation is painful and laborious, especially to the most distant eye, they soon relax one of the eyes, and content themselves with examining it with the eye that is next it; whence arises a diversity of situation, and a habit of moving the eyes differently.

In this case, which may admit of a cure, if not too much confirmed, it is evident, that objects will be seen in the same place by both eyes, and therefore must appear single as to other men; but because, in the eye that squints, the
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image of the object to which the other eye is directed, falls not on the most sensible and delicate part of the retina, which is naturally in the axis of the eye, it is easy to see that it must be but faintly perceived by this eye. Hence it is that, while they are attentive in viewing any object, if the hand be brought before the other eye, this object will be but obscurely seen, till the eye change its situation, and have its axis directed to it; which change of situation is indeed very easy for them, because it depends on the muscles of the eyes, whose functions are entire, but, by reason of the habit they have contracted of moving their eyes differently, the other eye is at the same time frequently turned aside, so that only one at a time is directed to the object.

That all this may be the better perceived, for an object, cause them to look at the image of the upper part of your nose in a plain mirror, while you stand directly behind them, to observe the direction of their eyes.

Secondly, The strabismus may proceed from a fault in the first conformation, by which the most delicate and sensible part of the retina is removed from its natural situation, which is directly opposite to the pupil, and is placed a little to a side of the axis of the eye, which obliges them to turn the eye away from the object they would view, that its picture may fall on this most sensible part of the organ. Thus, if *a* (See fig. 1.) be supposed to be the most sensible part of the retina, in order to see the object *H*, the eye must be turned aside to *F*.

When this is the case, the disease is altogether

gether incurable, and the phænomena that arise therefrom differ in nothing from the phænomena of the former case; excepting only that here, *1mo*, The object to which the eye is not directed will be best seen, which is the reverse of what happens when this disease arises barely from habit and custom. *2do*, No object will appear altogether clear and distinct; for all objects to which the eye is directed, by having their image painted on the retina at the axis of the eye, where it is not very sensible, will be but obscurely seen; and objects that are placed so far to a side of the optic axis, as is necessary for making their image fall on the most sensible and delicate part of the retina, must appear a little confused, because the several pencils of rays that come therefrom fall too obliquely on the crystalline, to be accurately collected in so many distinct points of the retina; tho' it must be acknowledged, that this confusion is for the most part so small as to escape unobserved.

Thirdly, This disease may proceed from an oblique position of the crystalline, as in fig. 5. where the rays that come directly to the eye from an object at A, and that ought to converge to the point of the retina D, which is in the axis of the eye, DE, are, by reason of the obliquity of the crystalline, made to converge to another point, as C, on that side of the visual axis DEA, where the crystalline is most elevated; and therefore the object is but obscurely seen, because its image falls not on the retina at the axis of the eye, where it is most sensible. But the rays that fall obliquely on
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the eye, as those that come from an object at B, will, after refraction, converge to this most sensible part of the retina D, and, by converging there, must impress the mind with a clearer idea of the object from whence they came. It is for this reason that the eye never moves uniformly with the other, but turns away from the object it would view, being attentive to the object to which it is not directed. When this is the case, it is in vain to expect any good from medicine.

The symptoms that naturally arise from it are, *1mo*, The object A, to which the eye is directed, will be but faintly seen, because its image falls on the retina at C, where it is not very sensible. *2do*, The object B, to which the eye is not directed, by having its image painted on the retina at the axis of the eye DE, will be clearly perceived. But, *3tio*, This same object B must appear somewhat indistinct, because the pencils of rays that flow from it are not accurately collected in so many distinct points in the retina, by reason of their oblique incidence on the crystalline. And, *4to*, It must be seen, not in its proper place B, but thence translated to some other place, as A, situated in the axis of vision DEA. (See the preceeding lemma and scholium.) And, *5to*, Being thus translated from its true place, where it is seen by the other eye that does not squint, it must necessarily appear double, and the distance between the places of its appearance will be still greater, if the crystalline of the other eye incline to the contrary side.

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Had Mons de la Hire attended to this translation and double appearance of the object, I see not how he could have imagined, that, when the crystalline is inclined differently in both eyes, the moon, for example, will appear like two ovals that decussate one another, as in Fig. 6. (See his dissertation *sur les differens accidens de la vue*). For, though the moon or any round body may appear a little oval from the unequal refraction of the rays in the oblique crystallines, and though these ovals may have different inclinations arising from the different inclinations of the crystallines; yet they can never decussate one another, but, being translated to two different places, must necessarily appear double, and at a distance from each other.

Fourthly, This disease may arise from an oblique position of the cornea, which, as Maitre-Jean observes, in this case is commonly more arched and prominent than what it is naturally. When the eye has this confirmation, no object to which it is directed can be clearly seen, because its image falls not on the retina, at the axis of the eye, and therefore the eye turns aside from the object it would view, that its image may fall on the most sensible part of the retina.

To determine the situation of the eye, with respect to the object it would view, let AGK be the axis of vision, (See Fig. 7.), and let the arch bGd, whose center is a, represent the oblique prominent cornea, and let aAx be a cone of rays, having its basis in the pupil, and its apex in the most sensible part of the retina;

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retina; it is evident that this cone must come from a point without the eye, which of all others appears most clear and distinct. To find therefore the situation of this point, nothing is required but to trace back the middle ray AG, in the line it described before its incidence on the cornea at G. For this purpose erect upon the surface of the cornea, from the point of incidence G, the perpendicular GP, and having produced it downwards to Q, from A let fall upon it the perpendicular AD, and produce it to H; so that DH may be to AD as the sine of incidence to the sine of refraction, that is, as 4 to 3; and about the centre G, with the radius GA, describing a circle AHP, draw, parallel to the perpendicular GPQ, the line HE cutting the circumference in E, and join EG; this line EG shall be the line of the incident ray, in which an object must be placed to have its image painted on the retina at the axis of the eye A, where it is most sensible. For, if EF be let fall perpendicularly on the line PQ, this line EF shall be the sine of incidence of the ray EG, the angle of incidence being EGP, and this sine EF is equal to DH, and consequently in proportion to the sine of refraction AD, as 4 to 3.

When the strabismus proceeds from this cause, the prognostic and phaenomena that attend it will be much the same as in the case immediately preceeding, from which nevertheless it may be distinguished by the obliquity of the cornea, which is manifest to the senses; and, if the cornea be also more arched and prominent than what it is naturally, which is

commonly the case, the eye will also be short-sighted.

Fifthly, This want of uniformity in the motions of our eyes may arise from a defect, or any great weakness or imperfection in the sight of both, or either of the eyes.

It will afterwards be made appear, that the motions of our eyes are voluntary, and depending upon our mind, which being a wise agent, does will them to move uniformly; not from any absolute necessity in the thing itself, or for want of power to move them differently, but because of the utility and advantage that arises from such motions. Hence children, for some time after birth, may be observed to look differently with both eyes, till, from the advantage they find in moving them the same way, they come at length to move them always uniformly; which uniform motion does at last become so necessary, as to render any other motion impossible, it being certain that custom and habit does render many actions necessary which are not essentially so.

This being the case with respect to the uniform motion of our eyes, it follows, that, when the sight of both or either of the eyes is so defective, weak, or imperfect, as to disappoint us of the advantages that naturally arise from their uniform motion, the mind will not accustom itself to this sort of motion, but will preserve its natural and connate liberty of moving them differently. An example of this may be had in those who, from a cataract or *gutta serena*, have been blind from the birth in both or either of their eyes; and that the same

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same thing may also happen when the disease is of a later date, seems evident from what Plempius observes (*Ophthalmograph. lib. iv. problem 13.*) of a girl who became blind from a *gutta serena* in the fifth year of her age: This girl, finding no further advantage, after being blind, in moving her eyes the same way, came at last to forget the habit she had formerly acquired in the four years she saw, and moved her eyes differently, sometimes their axes being parallel, sometimes converging, and at other times diverging.

The prognostic in this case is the same with that of the disease from which it proceeds, and the phaenomena are so obvious from what has been already said, that to say any more, would be but to make repetitions.

Sixthly, Another cause from which the strabismus may proceed, lies in the muscles that move the eye. When any of those muscles are too short or too long, too tense or too lax, or are seized with a spasm or paralysis, their equilibrium will be destroyed, and the eye will be turned towards or from that side where the muscles are faulty.

In this case, the disease frequently yields to medicine, and therefore admits of a favourable prognostic; excepting only when, by a fault in the first conformation, any of the muscles are longer or shorter than their antagonist; in which case, if ever it should happen, no medicine can be of any value.

As to what concerns the optical phaenomena, they are the same here as in case first; only, when the disease commences not till, by custom

from and habit, the uniform motion of the eyes has been rendered necessary, all objects do for some time appear double, of which we have given some examples above.

To explain how this should come to pass, and at the same time to reconcile it with what has been formerly said, from *p.* 158. to *p.* 195. concerning the single appearance of objects seen with both eyes, where I have demonstrated that this single appearance does not depend upon the uniform motions of our eyes, is a matter of very great difficulty, and therefore deserves well to be very carefully examined.

It hath been shewn that objects are seen without the eye, in lines drawn perpendicularly to the retina, from that point of it where their image is painted; it hath likewise been demonstrated, that these lines continue invariably the same without any change of situation, though the eye be turned away from the object to which the other eye is directed: Whence it seems to follow, that the object in all situations of the eye should continue to be seen in the same place by both eyes; and being seen in the same place, should always appear single: How comes it therefore that, in the case before us, the object should appear double? What is it can put this cheat upon the understanding? Whether the eyes look the same way or differently, the object appears to each of them in the same lines; and since, at the same time, we also perceive its distance, it ought to appear single, because seen in the concurrence of these lines, and yet it is esteemed double: Herein consists the difficulty, which does vanish and
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admit of a most easy solution, if we but consider that the mind mistakes the situation of the eye, and supposes that it is directed to the same object with the other; for, having been accustomed to move both eyes uniformly, from which a kind of necessity has arisen that makes it impossible for us to move them differently, when we would view any object, the mind wills both eyes to be directed towards it, and, not being sensible that either of them has disobeyed its commands, it concludes that they are both turned the same way, because it has been accustomed to find them so upon the like occasions: And being thus mistaken as to the situation of the eye, it must also be mistaken as to the situation of the object seen by it, which therefore must appear double, by reason of the different situation it appears in to each eye, it being impossible for us to conceive that any one thing can be in two different places at the same time.

To illustrate this matter, let it be supposed, that one wills both eyes to be directed to F, (See Fig. 1.), for viewing it accurately, and that, while the left eye gives ready obedience, let the other, by reason of a recent defect in some of its muscles, be turned to H: It is plain that the point F will be seen in the same perpendicular line CF it would have appeared in, had it been directed to F; but, because this line, by reason of the obliquity of the eye, does not fall on the retina at its axis C, but at some other point on the outside of this axis as E, so that the angle C α E may be equal to the angle F α H, it will itself appear translated to EG,

EG, and being thus translated, the point F must be translated with it, which therefore will be seen, not in its proper place F, but in some other place as G, situated in the perpendicular EG. For, since the mind knows not but the eye is directed to F, it must form the same judgment with respect to the situation of objects, as if it were really so: But it has been already shewn, that objects are always seen by virtue of a connate immutable law, in lines drawn perpendicularly to the retina, from that point of it where their image falls; and therefore the object F, having its image painted on the retina at E, must be seen by this eye, which the mind supposes directed to F, somewhere in the perpendicular EG, as at G; while to the other eye it appears in its true place F: And, being thus seen in two different places, G and F, it must necessarily appear double.

Something like this happens when an object is felt, by the extremities of two fingers that are made to cross each other; for the mind, not attending to the position of the fingers, judges that the object is double; because it is felt by those parts of the fingers that use to be at a distance from each other.

That we have here given the true account of this phænomenon, will be further evident to any one who considers, that, when the mind does not mistake the situation of the eye, as in those who by custom have from their infancy contracted a habit of moving their eyes differently, all objects appear single as to other men; and this likewise is the reason why, in the case before us, all things come in time

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to be seen single: For, by repeated experiences, the mind becomes wiser, and, by degrees, learns to form a right judgment concerning the direction of the eye, which judgment, by becoming habitual, must serve to correct the mistake it was formerly led into with respect to the situation of objects. The ingenious Mr Cheselden gives us a very beautiful example of this in his system of anatomy; his words are "A gentleman who, from a blow on the head, had one eye distorted, found every object appear double; but, by degrees, the most familiar ones became single, and in time all objects became so, without any amendment of the distortion."

The not considering of what has been said, has been a fundamental and perplexing oversight; for proof whereof, we need go no farther than the case before us; for explaining which many learned men have been forced to suppose, that by sight alone we can never form any judgment with respect to the situation of objects, without calling in the experiences of touch, the contrary of which has been set forth at large, and demonstrated in the preceeding scholion.

From all that has been said on this head, laid together and duly considered, we may clearly deduce this inference: The double appearance of objects that happens when either of the eyes is, from a spasm or paralysis of any of their muscles, or from any other cause, restrained from following the motions of the other, does not prove, that, to see objects single, it is absolutely requisite that both eyes be directed

rected to the same object, and that this is one of the final causes of their uniform motion.

Lastly, This want of uniformity in the motions of our eyes may proceed from a preternatural adhesion or attachment to the eye-lids; of this we have already given an example from Langius. And that the same thing may also be occasioned by a tumour of any kind within the orbit, pressing the eye aside, and restraining it from following the motions of the other, is so evident, that I need not bring instances to prove it; those who desire such, may consult observators. Here also the case may admit of a favourable prognostic: And as for what concerns the optical phænomena, they must be the same as in the case immediately preceeding.

Having finished what I intended to say concerning the final causes of the uniform motion of our eyes, I come now to inquire into the efficient cause of this uniformity, or by what necessity it happens that both eyes are always turned the same way, so that none of us are able at pleasure to give them different direction*.

Aristotle of old, and after him Galen, Avicenna, and most of the antients, do attribute this to the union of the optic nerves, near the *fella ossis sphenoides*; but, since these nerves give no branches to the muscles, but are wholly bestowed upon the retinae, it follows that they can attribute nothing towards the motion of our eyes, but are only for conveying to the mind, or visive faculty, the impressions made upon their fund by the rays of light.

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Hence it is, that, in blindness from obstructions in those nerves, the eyes continue to move as formerly; because their motion does not depend upon the optic nerves, but upon their other nerves and muscles. But, supposing that the optic nerves did contribute to the motion of our eyes, yet their conjunction could never occasion this uniform motion; because, as Diemerbroeck observes, (See his Anatomy, *lib. iii. cap. 16.*), anatomists have found them disjoined in some subjects, who, while alive, moved their eyes uniformly as other men.

It is therefore, with good reason, that our moderns have rejected this hypothesis as false and groundless; but neither have they themselves succeeded better, when they tell us, That this happens because the nerves bestowed upon the muscles of our eyes, called *oculorum motorii*, are united at their origin in the brain. Every body knows that our fingers are at liberty to execute different motions, and to be extended separately, though not only the nerve, but also the muscle subservient to their extension is but one: Whence therefore this liberty should be denied our eyes, whose muscles are distinct, I see not. But this is not all; for there are many parts of the body, which, though they have nerves of different origins, yet they necessarily move together. Thus the eyes cannot be turned up or down, but the eye-lids follow their motion, and keep at the same distance from the pupil, though at the same time the eye-lids can be moved without any motion in our eyes. Did this uniform motion depend upon any union or

conjunction of the *oculorum motorii*, or of any of our other nerves, none would squint but such as had them disjoined; and it would be in vain to use any precaution against childrens taking up such a habit, or to endeavour to correct it.

The true cause of this uniformity in the motions of our eyes to me seems wholly to depend on custom and habit. For it is not to be doubted but these motions are voluntary, and depending upon our mind, which, being a wise agent, wills them to move uniformly, not from any intrinsic necessity in the thing itself, or for want of power to move them differently; but because such motions are most profitable and useful to us. So that our opinion is, that the uniform motion of our eyes is not at first necessary, but that the mind has imposed upon itself that law founded upon the utility and advantage that arises from this sort of motion; which motion does in time become so necessary, that none of us are now able to move one eye towards any object, but the other is likewise turned the same way. And, as for other creatures who move their eyes differently, such as the chameleon, which has this faculty in an eminent manner, so that the one eye is moved whilst the other remains immoveable; the one is turned forwards, at the same time the other looks behind; and the one looks up to the sky, when the other is fixed on the ground: I say, as for other creatures that move their eyes differently, such as the hare, chameleon, &c. it is evident, since the organs subservient to those motions are the same as in man, that it is the utility and advantage they receive

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receive from these particular motions, which determines that principle which governs and rules all their motions, to actuate the organs in such a manner as those motions, which they find most profitable and necessary for them, may follow.

Dr Goddard (in the Philosophical Transactions) has observed, that the eyes of the chameleon resemble a lens or convex glass, set in a versatile globular socket, of which our Parisian academists have taken no notice. But, be this as it will, they found that they were moved by true muscles, which, as in other creatures, are inserted under the conjunctiva; so that it seems Panarolus was mistaken, when, as Bartholin informs us, (*Hist. anat. rar. cent. 2. hist. 62.*) he says, that their eyes want muscles, and that they are moved by the corrugation of a membrane, which is contracted by means of circular fibres. What might have led him into this mistake, may be guessed at from the observations of the same academists, who tell us, that the eyes, which are very large, jut out full half of their ball, and are covered with one single eye-lid, made like a cap pierced through the middle, with a hole not exceeding one line in breadth. This eye-lid was found fastened to the fore part of the eye, by means of an orbicular muscle that was spread over the whole *tunica conjunctiva*, to which, as well as to the eye-lid, it was so adherent, that it served to give the same motion to the lid as to the eye, though its particular action was to contract the little round hole of the lid, which closes by enlarging itself cross-

ways, even to the making one single slit, which very exactly unites the upper part with the lower. Seeing then that the eye cannot be moved, without communicating the same motion to the eyelid, which must therefore appear corrugated, it is probable that Panarolus, for want of due scrutiny after the muscles, might have imagined that the motions of the eye proceeded from the corrugation of this membrane, which is indeed contracted by means of the circular fibres of the orbicular muscle. But, supposing Panarolus's observation to have been just, it is all one with respect to the present case; for the dissimilar motion of their eyes, arising from the dissimilar contraction of those circular fibres, can have no foundation, but in the utility and advantage that arises from such motions, which might as well have been executed by muscles. Nor can any good reason be assigned why the mind, which presides over all the animal (if not also the vital and natural) motions, should not be at liberty to contract this or that muscle independently of others, as well as to contract this or that fibre independently of others, especially when we find it frequently does so in other creatures, such as fishes, birds; and amongst quadrupeds, the hare, coney, &c.

And as the hare, chameleon, &c, have a power of moving their eyes differently, so neither is there any room to doubt, but that at first we ourselves are also possessed of the like power; as is evident from the observation, already mentioned, of children, who, for some time after birth, can look different ways with their eyes; which power they retain, till, by
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discovering the advantage of directing them the same way, they come to move them always uniformly. This uniform motion, by use and habit, at last becomes so necessary, that the eyes cannot be moved differently; long custom rendering many actions necessary, which were not so essentially, nor from the beginning.

I have already given an example of this in the motions of the upper eye lid, which always follows the motion of the eye, and keeps at the same distance from the pupil, whether the eye be turned up or down. The same thing may also happen the fingers; for, if one is not accustomed to move any of them but in conjunction with the rest, it will not be in his power to move them separately. Hence it is that most people cannot bend their ring-finger towards the palm of their hand, but the little one shall follow its motion. If any body desires more examples of this kind, let him try to elevate one of his eye-brows, while the other is depressed; let him try to dilate one of his nostrils, or one side of his thorax, while the other is contracted; or, if he can, let him contract the muscles on one side of his belly, while those on the other side continue relaxed. I remember a time when it was very difficult for me to shut any one of my eyes, while the other was open, which now is very easy for me, because I have accustomed myself thereto; and this frequently happens, even in the eyes themselves: For, if we accustom ourselves to direct them different ways, as boys do often in imitating those that squint, we shall

in time be able to squint without difficulty, especially if young. This is further confirmed from the dissimilar motions of the eyes that are so frequently observed in blind people, and particularly by that history, already quoted from Plempius, of a girl who losing her sight, and having therefore no longer any advantage from the uniform motions of her eyes, came at last to move them differently. Hence it is that children, the younger they are, are the more apt to become gogle-ey'd; because, when young, they have not so much accustomed themselves to look the same way with both eyes, as to render that uniform motion necessary; and therefore do frequently become gogle-ey'd, by having many pleasant objects presented to them at the same time, which invites them to turn one eye to one object, and the other eye to another: And thus they contract a habit of moving their eyes differently, which is apt to continue all their lifetime, if not timely corrected. Willis has observed this (in his *Anima Brutorum*, cap. 15.) in these words: “Quare
 “infantes, quando ipsorum oculis multæ res
 “simul objiciuntur, strabismus facile contra-
 “hæret.”

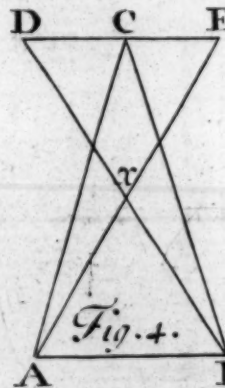
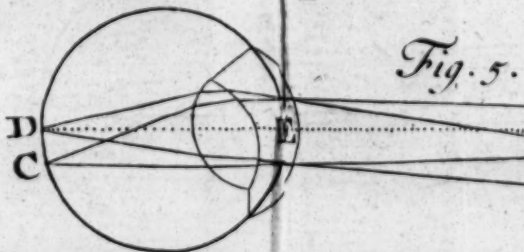
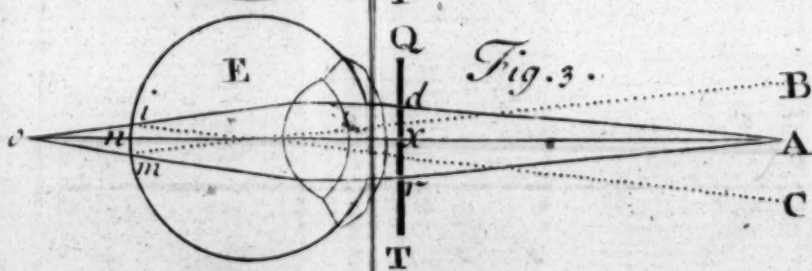
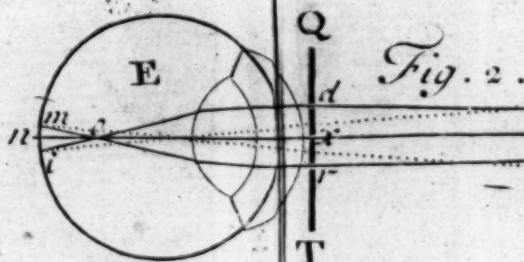
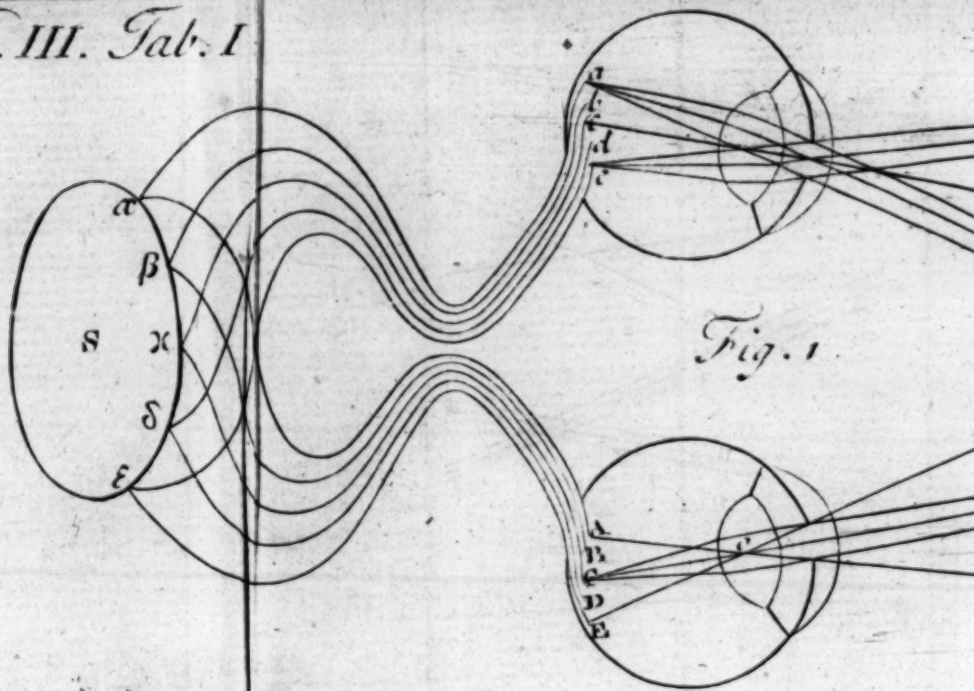
XIII *Supplement to Art. XI. of Vol. I.; by*
 ALEXANDER MONRO, *Professor of Anatomy in the University of Edinburgh.*

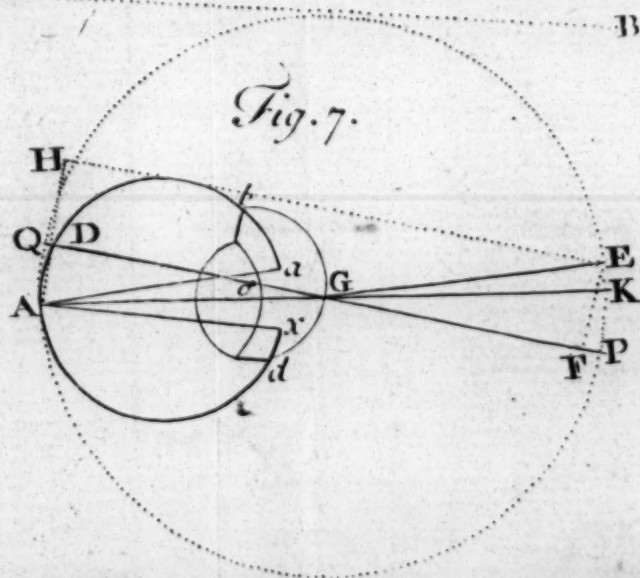
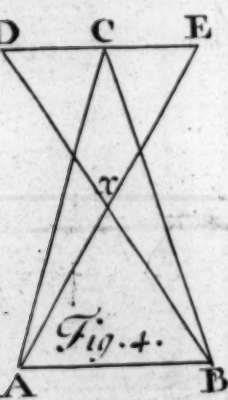
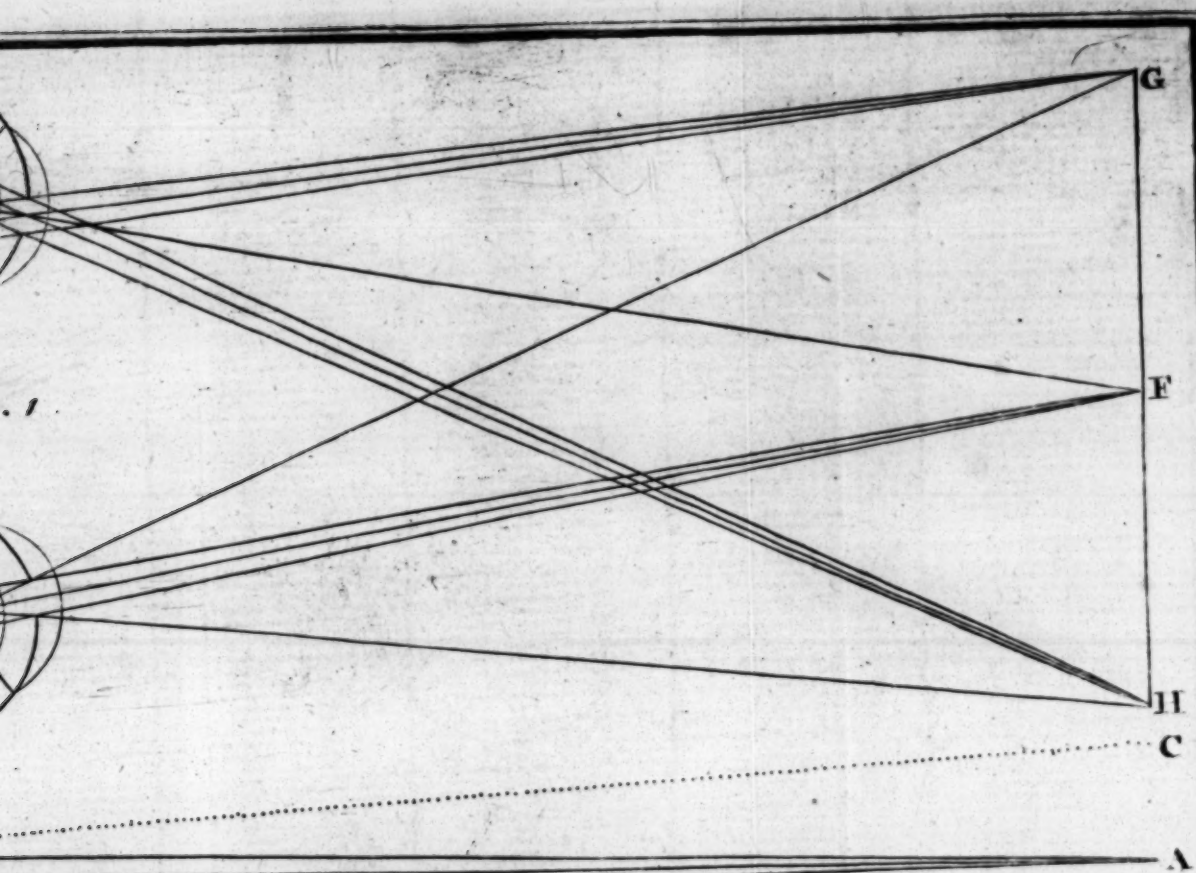
HAVING conversed with some younger anatomists, whose imagination could not supply the want of a figure to explain what I wrote

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wrote in Art XI. of your first volume, concerning the digastric muscles, I beg you would, on their account, publish the inclosed figures, with a reflection or two on them, as a supplement to that paper.

Tab. II. fig. 1. is a copy of as much of Cowper's tab. 23. *Myot.* as relates to the parts in question, with the addition of two or three pricked lines; the view here represented being a direct one of the left side, with the head reclined towards the right, and the muscles in an unactive state.

A, denotes the base of the lower jaw, to which the anterior head of the digastric muscle is fixed.

B, The part of the temporal bone from which the other head of the digastric rises.

C, The *os hyoides*.

D, The internal jugular vein filled with wax.

E, The anterior head of the digastric muscle

F, The posterior fleshy belly of the same muscle.

G, g, The middle tendon common to both these heads.

H, A strong aponeurosis sent from that tendon to the *os hyoides*.

i, Part of that tendon raised into a convexity forwards, by the over-distended jugular vein.

K, The *stylo-hyoid* muscle, through which the tendon of the digastric passes.

L, The extremity of the *stylo-hyoid* muscle, that is fixed to the *os hyoides*.

Fig.

Fig. 2. Represents the direct anterior view of the *stylo-hyoid* and *digastric* muscles, with the *os hyoides*, when the head is drawn much back.

The letters common to this figure, with the former, denoting the same parts, a comparison of the two figures will shew what the effect of the different attitudes is.

Besides what is common to both, *q, q*, here point out the great curve of the round tendons, to be at the same part where the strong aponeurosis *H* goes off to the *os hyoides*.

P is the thin aponeurosis of the *digastric* muscles, spread upon the *mylo-hyoid* muscles.

You see then that Mr Cowper paints no ligamentous pulley here for the round tendon of the *digastric* muscle to move in; nor has he any in tab. 31. *Myotom.* where the *digastric* is again represented; but in both he delineates the aponeurosis fixed to the *os hyoides*, which he mentions in his description, num. 68. as a constant thing, as I always find it. But he is obliged to acknowledge (notwithstanding the fondness he expresses for being the first who shewed the mechanism by which the *digastrics* act on the lower jaw) that the tendon passes through the *stylo-hyoides*, and an annular ligament, or sometimes a membranous inclosure. I still affirm, that I never saw any ligament, such as ties down the muscles of the hand, fingers, foot, toes, or any other muscle, whose action is determined by a band, or ligament, to any direction different from the straight course of its fleshy fibres, and that what is called the annular ligament of the tendon of
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the digastric muscle, is no other than the common cellular membrane, which stretches with the least force drawing it, and readily rises into cells upon blowing air into it.

Some have fancied that the stylo-hyoideus muscle serves as a pulley to the tendon of the digastric; but any who have ever dissected muscles, must know that their fleshy fibres are connected to each other by such weak fibres, as can make very little resistance; and, in dissecting this part of the stylo-hyoideus muscle, we scarce can hold it with a hook or forceps so tense as to dissect it clean, without the risk of tearing its fibres asunder, which plainly shews it to be too weak for the use they assign it. They ought to have also considered, that the tendon has no curve where it passes through this muscle, which is a sure sign that the muscle does not serve as a pulley to it.

Though there was a pulley, through which this tendon passed, it could be of no use, so long as the tendon is tied to the *os hyoides*; for the aponeurosis, H, hinders its play in the same way as threads tied to a cord laid over a pulley, and to the screw or ropes by which the pulley is suspended, prevent motion of the cord.

It has been said, that, though there is a great curve in the middle of each digastric muscle, when the head is extended backwards, yet it becomes straight when the head is bended forwards; and that, when these muscles are straight, they may bring the lower jaw down.—To this it may be replied, 1. That the digastric muscles never are straight; there is a curve in them

them when the chin touches the breast. 2. If they were to pull the jaw down, they must draw the *os hyoides* upwards and backwards; whereas this bone descends and advances forwards when the mouth is opened. 3. In this situation of the head, bended so greatly forwards, which is supposed to be the proper one for the action of the digastric muscles, the mouth is not opened by the descent of the lower jaw, but by the upper one only being raised.

One may readily judge from the figures, what the action of this digastric muscle must be. If the posterior head, F, alone act, the middle tendon will be drawn backwards till the aponeurosis shuffles from *m* to *o*, which is a very small space in Mr Cowper's figure, and must be much less in mine, after which this head acts on the *os hyoides*.

The anterior belly, E, acting alone, the extremity of the aponeurosis, H, is brought from *m* to *n*, which is as much as this head could be expected to contract.

If both heads act while the *os hyoides* is not kept down, the muscle becomes straighter, approaching the line *r*, till its axis is placed in that line, which I have endeavoured to prove, is the proper office of this muscle in deglutition, in which it is assisted by the stylo-hyoideus, K.

If both heads contract while the muscles, which pull the *os hyoides* down, are in action, the tendon, G, will be raised towards *r*, till the aponeurosis, H, is made straight and tense. Both bellies then continuing to contract in this situation,

situation, they will conspire in their efforts to draw the *os hyoides* upwards; besides which, the anterior head pulls also the jaw, A. Before the jaw can be depressed, the *os hyoides*, C, must be so firmly kept down by its muscles, that it shall be more difficultly raised than the jaw can be depressed, otherwise E would move the *os hyoides* upwards, without any effect on the jaw. Since, therefore, the jaw can be depressed with no greater force than what is employed to pull the *os hyoides* down, it will necessarily follow, that, whatever diminishes the force pulling the *os hyoides* down, must hinder instead of assisting the depression of the jaw. But, in the present supposition of both heads of the digastric muscle, contracting in the situation above described, it is evident that the posterior head can have no effect on the jaw, but exerts its whole force in pulling the *os hyoides* upwards, by which it counteracts the muscles, *i. e.* diminishes the force that draws the *os hyoides* down, (which the anterior head cannot increase), and consequently the jaw is depressed, or the mouth is opened with less force, when the digastric muscle contracts, than when it is unactive: From all which, especially if joined to the experiments mentioned in the article to which this is a supplement, it appears evident to me, that the depression of the jaw is no more the office of the digastric than the extension of the fore-arm is the office of the *brachialis internus*.

ARticle xi. of vol. 1. and this its supplement, having been severely criticized without convincing me that I am in the errors imputed to me, I think myself obliged to offer what arguments I can in support of these articles, to which, on this account, no alterations are made in this edition, tho' my other papers in this collection have corrections and additions made to them.

My first critic is the famous Mr Winslow, one of the greatest masters in anatomy, to whom the world is infinitely obliged for numerous improvements and discoveries, and who appears, from his writings, to be a scrupulously honest man.

The second is Mr Ferrein, who has given several specimens of his accuracy and ingenuity published among the memoirs of the French academy of sciences, and in his other writings.

The third is professor Walther of Leipzig, who is author of numerous anatomical, physiological, and medical tracts, which shew him industrious and learned.

These gentlemen have argued so differently, that I must consider each by himself.

Answer to Mr WINSLOW.

MR Winslow * observes, 1. That at the end of vol. 1. of your medical Essays, &c.

* Men. de l'Acad. des sciences 1742. p. 178. of 4^{to} Edit. Or p, 239. of 12mo.

&c. published in 1733, notice is taken of his *Exposition Anatomique*, published in the year 1732.—2. That Dr Douglas's translation of the exposition was published in 1732.—3. That the 3d vol. of essays was published in 1735.

I fancy the meaning of these observations is, to insinuate that I was the first aggressor; and therefore, to set this matter in its true light, I must remark,

1. That our vol. 1. which is properly for June 1731—May 1732, was published in 1732 notwithstanding the booksellers having put 1733 upon its title page, according to the common practice of people of that business, when they publish books near the end of a year; and that this volume was stopped several weeks at the press, after more than two thirds of the sheets were cast off.—When my remarks on the articulation, &c. of the lower jaw, were read to the society in 1731, and when they were printed, I knew no more of the exposition, than that a system of anatomy was long and impatiently expected from Mr Winflow.

2. There must be a typographical error in the second observation; for 1734 is on the title page of Dr Douglas's translation of the exposition; nor could a translation of such a book have well been published sooner after the original.

3. When vol. 3. of essays was published, I did know Mr Winflow's opinion concerning the point now in dispute, but did not see reason to embrace it.

The observations are followed by an accurate account of Mr Winslow's doctrine, and then mine is attacked.

On my argument of the digastric muscles being too short for drawing down the lower jaw, in opening the mouth *, it is remarked (p. 185. 4to, 249. 12mo.) that the lower jaw being a crooked lever, a small motion at its angles must make a considerable one at the chin; and that the digastric muscles may be regarded as if their insertion was towards its angles; therefore a small quantity of contraction of the fibres of these muscles is only required.

All will agree, that the motions of the several parts of the lower jaw, and of every other bone, are in proportion to their distances from their common center of motion; and therefore the angles of the jaw move much less than the chin does: But this is no reason why the digastric muscles should be regarded as if they were inserted into the angles, when they are inserted into a part of the jaw which has a much larger motion than the angles have, in proportion to which motion they must contract; and the quantity of this contraction must be according to the length of their fibres, which I still think are too short for such an office. This is confirmed by the dissection of quadrupeds and birds, whose digastric muscles depress the lower jaw; for, in them, these muscles are not inserted into the fore-part of the jaw, but much nearer to its center of motion.

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* Edinburgh essays, vol. 1, art. xi. p. 109.

AND OBSERVATIONS. 219

In stating my second objection, Mr Winslow has twice omitted the word *proportional*, and from thence is led to criticize me (p. 187. 4to, 251. 12mo) for what I have not said.

I have so good an opinion of Mr Winslow's integrity, that I shall give this hint, without entering into any argument, believing that it will be sufficient to make him wish these remarks had been omitted.

In answer to this 2d objection (p. 187--252) and in some other parts of his *memoire*, Mr Winslow also interprets what I called the angle of insertion of the digastric muscles, as if I meant by it the curvature of the tendons of these muscles.

What has occasioned this mistake, I cannot tell; for I have always named them differently: And I thought every one had known what is meant by the angle of insertion of a muscle into a bone, to wit, the angle between the muscle and the bone, and that the nearer this angle is to a right one, the muscle always acted, *ceteris paribus*, with greater advantage.—In the present question it might be easily demonstrated, that the nearer perpendicular a muscle fixed at one end to the *os hyoides*, and to the chin at the other end, is to a line drawn from the axis of the jaw through the part of the *os hyoides* where the muscle is fixed, the larger angle it must act with in drawing the jaw down; consequently the digastric muscles act with a larger angle when the head is reclined back, and with a less one as the chin is depressed; that is, these muscles have a less angle of insertion, as they continue to act; whereas most other muscles obtain a lar-

ger angle of insertion as they act. Thus, when the fore-arm is extended, the biceps and *brachialis internus* are inserted at a very acute angle; but, as the fore-arm bends, the angle of their insertion increases.

Whether the aponeurosis of the digastric muscles is long and loose enough to shuffle as much as is sufficient for their action, if they were depressors of the jaw, is the next point in which Mr Winslow and I differ, and which other anatomists inspecting the parts must determine.

After this I am blamed (*Mem.* p. 189—254.) for considering the actions of each head of the digastric muscles separately; and, by repeating this critical remark afterwards, Mr Winslow seems to think that these muscles always act simultaneously, or at the same time.

If I had neglected to consider the simultaneous action, I had been to blame; but, as this was done, there could be no fault in examining what each head can do; because I have no doubt of their often acting separately, as I see different parts of other muscles act, which seem to be more made for co-operation than these two parts of the digastric muscles. Thus, for example, when the arm hanging by the side, is to be raised by the deltoid muscle, its middle part only can act at first, because the fore and back parts of this muscle being then placed below the center of motion of the joint of the arm, might, if they acted, pull down the arm with as much force as the middle part could exert to raise it; and, on the contrary, when the arm is to be pressed to the ribs, the lateral parts of the deltoid muscle contract, while the middle part is inactive.

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In relating my 4th objection (*Mem. p. 189—254.*), there are two mistakes in translating my words which have given ground to criticism. Where I say the *os hyoides* is kept firm, or a little brought down, *brought down* is translated *poussé en embas*, pushed down; whereas the evident meaning in French is *abaissé* or *tiré en embas*.—Instead of translating the English words, *is the only supposition to be allowed*, by the French words *est la seule chose que je suppose*, the translation ought to have been *qu'on doit toujours supposer*.

After Mr Winflow has mentioned how people may be deceived in making trials of the actions of the muscles in dead bodies, he blames me (*Mem. p. 190.—256.*) for reclining the head backwards, when I was to examine the digastric muscles, and says that I ought to have made the dissection with the head in the ordinary erect posture.

In making these experiments, I did not free the muscles from any attachment which could have effect on their actions: I did no more than lay their external side so far bare as I could take hold of it with a hook, or put a needle into it; and I still think the experiments ought to be made with the head reclined backwards, rather than in any other posture, for this obvious reason, that it is in the reclined posture only that living people, evidently and without a dispute, cause their lower jaw to descend in opening the mouth; and therefore, if the digastric muscles ever have any effect in pulling down the jaw, it must be in this attitude. I made the trials in all the postures; but mentioned

this one particularly, as being the most decisive of the question in dispute.

I know not what use Mr Winslow intends should be made of his observation, that, in the most erect posture, "the middle curvature of the digastric muscles, called their *angle* by Mr Monro, is much less than the ordinary manner of dissecting has given occasion to attribute to them." It is certain that these muscles never are straight in any posture, and that Mr Monro never gave the name of *angle* to their curvature; and, in the erect posture, I see nothing in these muscles to persuade me, they are fit for pulling down the jaw, unless the experiment is made, as Mr Winslow has sometimes done, after taking away the adhesion of the middle tendon of a digastric muscle to the *os hyoides*. (*Mem. p.* 180.—242.)

I am also at a loss to know, what advantage Mr Winslow can have in the present argument, by considering the lower jaw as a crooked lever, as he has done (p. 185. & 191. 4to, 249. 257. 12mo): For a power acting on or drawing one end of a beam, the other end of which is the centre of motion, has the same force or effect, whether the beam is straight or crooked, provided the power acts in both with the same angle of insertion, and same distance from the centre of motion.

It is acknowledged (p. 192.—258.), that, when the head is reclined back, and the *os hyoides* is kept firm, "it is certain the posterior heads of the digastric muscles cannot act, and consequently can do nothing for the motion of the lower jaw:" But then, as the keeping
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firm or bringing down, or, as Mr Winslow is pleased to quote me, the *pushing down*, the lower jaw, is said to be only a supposition which is not to be found in a natural state, as is afterwards to be demonstrated, the conclusion which might otherwise be drawn cannot, it is said, be made.

There is no such demonstration, as Mr Winslow here promises, to be met with in this *memoire*, and the keeping firm or bringing down the *os hyoides*, when the mouth is opening, is so far from an imaginary supposition, that every person, putting a finger on the *os hyoides* while he opens his mouth, can be certain of its being a constant evident fact; and therefore Mr Winslow himself is here excluding the posterior heads of the digastric muscles from any share in the motion of the lower jaw downwards.

Unless the keeping the *os hyoides* down is again mentioned as an objection to my 5th section, there is none made (p. 192.—258).

When Mr Winslow knows, that, in the original of my 6th objection, the person endeavouring to feel whether the anterior heads of the digastric muscles are in action, is desired to do it while the mouth is opened ever so wide, quickly, or *strongly*, which last word is omitted in his quotation of this objection, he will wish some of his reflections, which depend on his omission of this word, had not been inserted.

It is admitted (p. 195 — 262), that the action of other muscles besides the digastric, is to be felt while the mouth is opened; and (p. 196.—264.) that there is a difficulty in feeling the action of the digastric muscles, though, it is alledged,
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it may be felt when the points of the two thumbs are put at the internal or posterior side of the bone of the chin, and resistance is made by the thumbs to the depression of the jaw.

In swallowing, any person may feel the anterior heads of the digastric muscles in action, without having accurate knowledge in anatomy: I desire only a comparison to be made, between the feeling of these muscles when even the least forcible deglutition is performed, and when the most straining effort is made to open the mouth. In deglutition these muscles are tense, hard, and swelled; in opening the mouth, their action cannot be distinguished: They then feel no more tense than the traction of the *os hyoides*, then drawn downwards, would make them, nor more protruded outwards than the swelling and rising outwards of the muscles above them could cause.

In proof the posterior heads of the digastric muscles acting in opening the mouth, it is said (p. 196---265.), that, "when the fore-finger is pushed between the lower part of the mastoid process and the angle of the lower jaw (as also a little more forwards), and then the finger is pressed strongly, while the jaw is drawn down with effort to the last degree, the belly of the digastric muscle of that side may be felt very distinctly as a kind of stretched cord, especially, if at the same time the chin is born up by the thumb of the other hand, to resist the effort of drawing down the jaw."

I did not expect Mr Winslow would have insisted on this posterior head acting in opening the

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the mouth, after acknowledging it certain that, "if the *os hyoides* is kept firm, (which it most undoubtedly is) this posterior head can do nothing for opening the mouth in the reclined or extended attitude of the head, in which the mouth is only opened by the descent of the lower jaw;" and when there cannot be so much as a pretence for the favourite bascule, or reversion of the upper part of the head upon the condyles of the maxilla. Since, however, he appeals to this trial, I must observe, that, in making it, no tense cord is felt in some people; in many it is felt, but in these it is very different from what is felt when deglutition is performed, both in its form and situation. In this latter action, one can distinguish the tension of the tendon, and the swelling of the belly of the muscle: In opening the mouth, the cord is higher, deeper, and without any perceivable swelling in one part more than another; and, therefore, I understand it to be the ligament extended from the styloid process of the temporal bone to the *os hyoides**, sustaining the *os hyoides*, or that it is the stylo-hyoideus muscle performing that office, when there is no such ligament, as frequently is the case†.

The last argument in the *memoire* for the digastric muscles drawing down the lower jaw in opening the mouth, is (p. 182.--245 p. 191 --257. p. 197---265.) the dissection of quadrupeds and birds, in whom the muscles, analogous to the human digastric, are not connected to

* Ruysch advers. dec. 3. § 9 Cowper Myot. in fol. Tab. 24. e Tab. 27. Fig. 1. DD.

† Weitbrecht Syndesmolog. Sect. vi. § 15.

to the *os hyoides*, but are extended in a straight course to the lower jaw, to the middle or back part of each side of which they are fixed, and evidently draw it down.

I agree with Mr Winslow both in the anatomy and uses of these muscles in quadrupeds and birds, but will not admit his conclusion, that the human digastric muscles have the same office; for these muscles of these creatures have not the curvature in the middle nor the connection to the *os hyoides*, nor the insertion into the chin, which the human digastric muscles have, and which are the circumstances upon which my objections to the common account of the action of these human muscles are founded.---If any person was to argue thus: The human digastric muscles are principal instruments in deglutition; therefore the analogous muscles of quadrupeds and birds are also organs of deglutition, Mr Winslow would refuse this conclusion to be just, for the same reasons as make me not to admit his conclusion.

After objecting to the common doctrine concerning the digastric muscles, I took notice of both jaws contributing to the opening of the mouth; the upper part of the head only moving when the chin is near the breast; the lower jaw only moving while the head is extended fully back, and both jaws moving in all the intermediate situations of the head, each of them moving nearly proportionally to the greater or lesser flexion or extension of the head. In proof of this, I desire any person to put a knife, or his nail, opposite to the conjoined edges of the teeth, before the mouth is opened, to hold the
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knife or nail unmoved, while, standing before a mirror, he opened his mouth in all these different attitudes of the head, and then he might see these motions of the jaws.

In the reflection on these experiments (*Mem. p. 198—267.*), Mr Winslow seems to think that, "when there is not space enough to make a large opening of the mouth by the descent of the lower jaw, one is naturally prompted to raise or recline the head backwards proportionally to the extent of space necessary to make the opening of the mouth by the ordinary depression of the lower jaw; and that it is thus that crocodiles raise the head to make a large throat, according to the observations on three crocodiles related in the collection of old *memoires.*"

If, in opening the mouth, while the chin is near the breast, all the head was first raised, and then the lower jaw descended, there would be some reason for this reflection; but, as the lower jaw is not in the least raised, but keeps its situation, while the upper part of the head only moves, it cannot invalidate what I have said; and there cannot be any intention of making space for the descent of the lower jaw, when this jaw is not to descend.—The Academicians, whom Mr Winslow quotes, evidently thought of crocodiles raising their upper jaw and cranium by the muscles placed on the back part of the vertebræ of their back and neck.

In the same reflection (*p. 199—268.*) it is said, that, in the experiment I proposed for seeing the motion of the jaws in a mirror, the person extends his head, that he may look into his throat:

—Whereas

—Whereas the experiment succeeds, whether the person sees or is blind, whether he knows for what purpose it is made, or is ignorant of it, whether he holds the knife himself, or another does it without his knowledge. In short, whoever makes the trials, or sees another do them with a mirror, or without one, must be convinced I have described what really is done in opening the mouth.

I am surprised the reflections above mentioned were insisted on, since they contradict the bascule, or reversion of the upper part of the head upon the condyles of the lower jaw, argued for (*p.* 181—243. *p.* 184—247.); which I would next examine, if it was not more proper to consider it in my remarks on Mr Ferrein's *memoire*, where it is more fully treated than in Mr Winslow's.

Whether the uses I have mentioned of the muscles below the *os hyoides*, to wit, the *sterno-hyoidei*, *coraco-hyoidei*, &c. are assented to or denied, (*p.* 195—262.) I cannot say; for tho' it is admitted, that "in great efforts to bring the lower jaw down to the last degree, and by this to open the mouth greatly, the tension, and a kind of hardness from this tension of these muscles are felt, yet it is said, that one shall not distinguish any hardness by the contraction of their fleshy fibres, nor any shortening by that contraction."

Any person putting his finger on the skin which covers these muscles, while he moves his lower jaw, can determine as well as the most accurate anatomist what these muscles then do; and therefore I shall appeal to every one's feeling for

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determining this part of the debate. My colleague Dr Rutherford, Mr Gibson surgeon at Leith, and I, were consulted about a tumor in the thyroid gland of a very lean lady, in whom the coraco-hyoid, sterno-hyoid, and sterno-thyroid muscles raised the skin covering them, on even the least motion made by her lower jaw in speaking to us, and then felt as firm hard cords under our fingers.

After Mr Winslow has finished his reflections on Art. xi. of your vol. i. he criticizes the foregoing supplement, but has nothing new in his remarks, except blaming me, (*p.* 202—272.) for making use of figures of the parts put in a wrong attitude, and particularly the one I had caused to be drawn of these parts, as they appear when the head is reclined back.

I have already said, and beg leave to repeat, that this is the most proper attitude in which the digastric muscles can be examined, in order to pass judgment on the dispute between us; because it is in this posture, that the opening of the mouth is solely made by the descent of the lower jaw; and therefore, if the digastric muscles are the organs which pull down this bone, then it is that their action must be greatest, and consequently most manifest.

I should have thought myself very happy in being prevented from causing any original anatomical figures to be drawn, which I probably would have been, had Mr Winslow given us the figures of all the parts of the body, as he intended; for, had the painters and engravers executed their parts with the same accuracy and candour, as he would have dissected and presented

the originals to them, we should have had by far the most compleat system of anatomy ever yet has appeared, though, as frailties must attend human nature, even a Winslow might have erred. Long may he live to do still more service to mankind; but I am afraid his great age will not bear the fatigue of such laborious work as these plates would require.

Answer to Mr FERREIN.

THE Academy of Sciences at Paris has published two *memoires* of Mr Ferrein, relating to the motions of the jaws, in their Transactions for the year 1744. In the first of which there is (*p.* 427. &c.—579. &c.) a description of the articulation of the lower jaw, agreeing with the one in Art. xi. of your vol. 1.; except that 1. (*p.* 427—579.) the substance interposed between the two pieces of which the lower jaw consists in infants, and between most other bones of the head, ought, it is said, not to be called a cartilage, as is commonly done, but a continuation of the periosteum.—2. (*p.* 329—580.) The substance which lines the cavity that receives the condyle of the lower jaw, is likewise said to be no cartilage, but a simple membrane.—3. (*p.* 430—582.) The elastic *meniscoid interarticular* cartilages are affirmed to be ligamentous bodies.—4. (*p.* 430—582.) The external and internal parts of the articular ligaments, which are thicker than the fore and back parts of these ligaments, according

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ing to the common rule, of the ligaments of the articulations by ginglymus being thicker at the sides to which least motion is allowed, are here esteemed four ligamentous cords.

Seeing self-defence alone obliges me to take up the pen at present, and none of these four specialities are particularly designed to criticize me, I shall enter into no dispute about them.

The motion of the jaw forwards, or its horizontal motion, is described in the *Memoire*, (p. 431—584.), in the same manner as in your *Essays*; but (p. 431—584. p. 432—586.) it is asserted, that authors have expressed themselves so obscurely concerning the motions of the jaw backwards, that it was impossible to know what they thought.

Whether is it impossible to understand the following words, (*Edin. Ess. vol. 1. p. 104.*) "The exterior extremity of this oblong posterior cavity is made very narrow by the *meatus auditorius externus* being considerably advanced forwards here, which again prevents the condyle ever to be pushed so far back as to be in hazard of doing any injury to the gland situated in the more internal part of the cavity?" Whether is not this a more particular account of the non-recession of the lower jaw, than to say in general, as is done in *Memoires* 1744, (p. 433—586.) that it is owing to the construction of the parts, and the opposition of the ligamentous cords?

Mr Ferrein, observing that the condyles are too much straitned in the glenoid cavities, for performing the large motions which the lower jaw makes to each side, (p. 434—588.) is astonished,

ed, "that an observation so simple, and a truth so striking, has escaped the eyes of so many anatomists."—Has he not overlooked the following words of the *Essays*, vol. 1. p. 106. "The condyles can be moved laterally when they are lodged in the cavities, or on the tubercles; but these lateral motions are much more confined in the cavities, because of the surrounding brims?"

Both anatomists and surgeons knew that the condyles of the lower jaw did not always advance and recede together, but were capable of being moved forwards or backwards, separately or alternately. They are, however, obliged to Mr Ferrein for his minute description of these motions, and their effects on the different parts of the jaw, (*p.* 435—588. *p.* 443—599.)

In *p.* 439.—594. Mr Ferrein has made a very pretty discovery, and has corrected an error into which all the anatomists had fallen, concerning the manner of the motion of the lower jaw in its depression and elevation.—The condyles had always been thought the axes upon which the lower jaw moved, as most other bones do upon their articulated ends; but Mr Ferrein has demonstrated the axis of motion of the lower jaw, in its motions upwards and downwards, to be a little above its angles; so that, when the body of the bone moves downwards, its condyles advance forwards, and when its body is raised up, the condyles recede back; by which, says he, the parotid glands are safe from compression, as the muscles which raise the jaw are from being overstretched; and it may be added, that the nerves and blood-vessels

which

which enter the internal posterior hole of the jaw, are likewise saved from being twisted, over-stretched, or torn, in the motions of this bone.

I shall conclude my remarks on this first *Memoire* of Mr Ferrein, by observing that this account of the aperture of the mouth and experience confirm what I have said, (*Eff. vol. 1. art. xi. p. 108.*) of the danger of a luxation of the lower jaw when the mouth is opened, and the condyles are on the tubercles of the temporal bones. It is in vomiting, coughing, yawning, &c. that these luxations ever happen, so that he should not have quoted this opinion as erroneous, (*p. 444—602.*).

THE first thing Mr Ferrein undertakes to prove, in his second *Memoire*, is, (*p. 511—687.*) "that the head contributes to make the opening of the mouth, and that it even contributes considerably, but by a motion independent of the will, and of the contraction of the splenius, complexus, and other muscles which raise the head; in a word by a motion reciprocal to that of the lower jaw, resulting from the effort that is made to depress the lower jaw, and occasioned by the resistance which it opposes to its depression."

I suspect I don't rightly understand this proposition; for I am at a loss to know what motion can be independent of the will, which results from an effort, and which is said, (*p. 540—729.*) to be an effect of the act of the will; and I am not certain whether the reciprocal motion here mentioned is Mr Winslow's bascule, or reversion of the head upon the

condyles of the lower jaw. I suppose it is ; and therefore am now to perform the promise I made in my answer to him.

As an introduction to Mr Ferrein's demonstration of the foregoing doctrine, he mentions the following law in mechanics : " I suppose
 " two bodies A and B, fastened to the two ends of
 " a cord which shortens itself ; it is evident that
 " the cord draws these two bodies, the one to-
 " wards the other, in making them pass over
 " spaces reciprocal to their masses, and that if
 " the mass A is double or triple of the mass
 " B, the space which the mass B moves shall
 " be double or triple of what A moves."

If the force with which the cord shortens itself is less than the resistance of the mass A, how much space would that Mass A move? None surely. This is the case of muscles, which almost perpetually act with a force which moves only the more moveable of the two parts to which their ends are connected. In proof of this I shall take Mr Ferrein's own examples of the fore arm and leg, which he has brought (p. 512—688.) to prove his mechanic law. — When the leg or the fore-arm alone is to be extended, its muscles move it only, so far as according to the rule I just now said is generally observed ; but, says Mr Ferrein, the reason why the thigh or arm don't move at the same time, is the resistance made to the motion of the thigh and arm by the almost enormous mass of the trunk, &c. which must be moved with them.—Let us suppose this a good reason, then observe what happens when one rises from a chair, if it is even on one foot, The exten-
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for muscles of that leg raise the enormous mass above it, and leave the leg immoveable; and in the same way one having his hands on a table with his fore-arms bended, can raise his body from the ground by the action of the extensors of the fore-arms, raising the arms and all the enormous weight of the body, the fore-arms remaining unmoved. If Mr Ferrein's law obtained in the action of muscles, and if a leg and foot, or the two fore-arms and hands, are one tenth or eighth of the weight of the whole body then, in these cases of rising from a chair, or raising our weight on our hands, the leg or fore-arms ought to move only one tenth or eighth less space than the thigh or arms move; whereas they move none. Thus muscles move only the least resisting part of the two they are connected to, though the difference of the resistance is but one eighth or tenth; nay, if a man rises with a weight in his arms, or on his back, the difference may not be one twentieth or thirtieth.

After assuming this mechanic law of muscles moving both the parts they are connected to, it is inferred, (p. 512—689.) that “the principles which are established, are certainly sufficient to demonstrate, that the effort which one makes to open the mouth ought to tend at the same time to depress the lower jaw, and to raise the upper one.”

Admitting the law in mechanics to have been demonstrated, and justly applied to the action of muscles, and granting every thing yet mentioned in this *memoire*, this consequence would not follow; because there has yet been no
proof

proof brought of a contractile cord extended from the head to the lower jaw.

In applying the law we have been disputing about, Mr Ferrein says, (p. 513—689.) “I have discovered, that, notwithstanding the head’s being nearer the trunk than the lower jaw, yet the resistance which the head makes to its elevation is extremely little; and such nearly as it would be, if the head was wholly separated from the spine. I have also found, that the resistance of the lower jaw, tho’ really less, (meaning, I suppose, than that of the head), is however considerable enough to occasion in the head the motion which I have attributed to it,” to wit, the elevation.

One must read the subsequent page of the *memoire*, before he can understand what the resistance of the head wholly separated from the spine is, which is here intended. It is, I suppose, the resistance to elevation which the head makes, when its upper part is made to move on the condyles of the lower jaw.

The heads of people falling asleep, or of children, and of weak persons, when they are in an erect posture, bend down their breasts, and whoever raises the head of a man in a faint, may find, that, in doing it, he must employ a considerable force, so that the resistance to the elevation of the head is not extremely little; and seeing it is here acknowledged, that the resistance of the lower jaw is less than that of the head, it will be concluded, from the foregoing experiments with the legs and fore-arms, and many more such might have been named, that, supposing a contractile cord to be extended from the

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the back part of the head to the lower jaw, this cord would only move the lower jaw, which is confessed to give the lesser resistance.

In the subsequent paragraph, there is an acknowledgment, which I heartily wish had been more attended to. The words of it are, "In truth, the reciprocal motion of the head upon the lower jaw cannot be made, if the processes articulated with the spine (these must be the condyles of the occipital bone) don't follow the determination which the motion of the head gives them."

The condyles of the occipital bone are the fulcra on which the head rests, and on which the motions of the cranium and upper jaw, independent of the spine and lower extremities, must always be performed. Whoever affirms, that the condyles of the lower jaw can be the fulcra and axis of these motions, might as well say, that a beam, AB, placed on the rigid pillar C, could ever have its axis of motion on the end D of another beam, DE, joined to AB by a hinge, where, it is evident, that, on raising the end, A, upon the axis, D, the other end, B, of the beam, must make its way down thro' the substance of the pillar, C, as the prick'd line, *ab*, does in Fig. 1. :—Or, if we would suppose, as in Fig. 2, that the beam, FG, supported by the pillar, H, is to have its end, F, moved downwards upon the axis, I, the end of a beam, KI, fixed to FG by a hinge; then it is evident, that FG must rise greatly from the end of the pillar, as the prick'd line *fg* does in the figure. It is almost needless to add, that the cranium, and upper jaw, are here represented by the lines AB,

AB, *ab*, FG, *fg*. The pillars, C and H, are instead of the spine, and the crooked lines, DE and IK, are in place of the lower jaw; on the supposition of whose condyles being the axes of motion of the head, the spine must be immensely compressible and elastic, to be lengthened and shortened so greatly, in the opening and shutting the mouth.

'Tis in vain to endeavour to elude the force of this objection to the reversion of the head upon the condyles of the lower jaw, by saying, as Mr Ferrein does, p. 514—690), That “the motion of the head on the first vertebra of the neck, and on the lower jaw, has nearly the same axis.”——Mr Ferrein knows very well, how much farther back the condyles of the occiput are, than either the glenoid cavities, or tubercles of the temporal bones, with which the lower jaw is articulated; for, of the space between the occipital condyles and the forepart of the jaws, the distance between these condyles and the middle of each cavity at the back part of the tubercle is about one fifth; and the distance between these condyles and the middle of the tubercle, is more than one fourth: But, if the occipital condyles are at all behind these parts of the temporal bones, it is absolutely impossible, that, while the head and spine are kept together, the fulcrum, or axis of the head can be on the condyles of the lower jaw, or indeed any where, but on the occipital condyles or spine: And Mr Ferrein seems conscious of this; for he does not affirm, that the axes are precisely the same, which they ought to be, in his and Mr Winslow's opinion of the motions

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tions being performed on the condyles of the lower jaw, but only affirms here and afterwards, (p. 517.—695.), that they are nearly the same.

The only way to make the head move on the lower jaw would be to take away its supporting pillar the spine, that the back part of the head might be left loose and free; and then to hold the lower jaw firm. Upon comparing the axis of motion, in these motions performed by the head in this situation, it may be seen very different from what it is while the spine supports the head, whether the vertebræ are put on a table, as Mr Ferrein (p. 514.—690.) proposes, or whether no part of the spine is separated from the body, which is much the better way of making this experiment: The axis of motion of the head is therefore the same, whether it moves with or without the opening of the mouth.

Mr Ferrein, aware that, though all his preliminary propositions were granted, yet the lower jaw must make a considerable resistance before the head could be raised, mentions, (p. 515.—692.) four causes which conspire to make this resistance, to wit, 1. The inertia of the parts, which, as in all matter, makes them resist motion.—2. The *ployment* of the parts which compose the fore-part of the neck.—3. The connections of the lower jaw, with the upper part of the neck.—4. The fleshy body of the pharynx, which is connected to the neck and lower jaw.

I am surprised that the first of these causes is mentioned to prevail in a pendulous member, whose gravity is making it tend downwards.—I did not translate *ployment* in the account of the second cause, because I cannot think this
word

word is here taken in the common signification of bending, sinking, shrivelling, or shortening, which would all contribute to bring down the lower jaw; and, if I was to take any notice of the 1st, or of this 2d, and the 3d and 4th causes, it would have been to enumerate them as causes which draw the lower jaw down; for they have not only gravity to cause them to descend, but the muscular contractile organs, on the fore part of the neck, being connected below to parts which are much more difficultly moved than the lower jaw, to wit, the sternum, clavicles, scapulae, diaphragm, whatever power of contraction they have must be a visus to draw down the jaw.

It is however needless, in the present argument, to insist on a minute classing of the causes, when we every day see the jaw falling, and the mouth open, in drunken, paralytic, dying persons, or whenever the levators of the jaw become weak, nay, whenever the attention of the mind is taken off from the office of sustaining the jaw, it falls. Thus people in profound meditation generally have their mouths open; and a country-fellow stands gaping, while he is gazing at the novelties of a town. — When the cheeks and the muscles, which are levators of the lower jaw, are cut in a dead body, the mouth remains open, while the head is in the erect posture.

I shall make no remarks on Mr Ferrein's proof of the possibility and reality of the motion of the upper jaw in opening the mouth (contained in *mem. p.* 515, 516, 517.—693, 694, 695) but beg the reader to compare them with a few lines in p. 115. of vol. 1. of Edinb. Ess.

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AND OBSERVATIONS. 241

This section of Mr Ferrein's *memoire* is concluded, p. 516.—695, with five remarks.

“ 1. The axis of motion in the depression of the lower jaw is, according to him, nearly the same with the axis in the elevation of the upper jaw.

“ 2. These axes are both very different from the axis of the natural motion of the head; such as is made naturally, and without constraint, by means of the splenius and complexus.”

Of the doctrine in these remarks, enough is already said.”

“ 3. The reciprocal motion of the upper jaw is independent of the contraction of the splenius, complexus, and other levator muscles of the head; this is demonstrated by reason and experience.”

I see no reason for this in the *memoire*, but I shall take one against it from Mr Ferrein's own doctrine, which is using what is commonly called *argumentum ad hominem*. In p. 541.—730. he gives it as a maxim, “ That every muscle being connected at its two ends to two different bones, every motion of the one or of the other of these two bones, which makes one of these ends to come nearer to the other, depends, at least in part, upon the contraction of that muscle.” Now, whatever axis the upper part of the head has in its elevation, Mr Ferrein will certainly grant, that the back-part of the head descends during the elevation of the fore-part of it; and this back-part cannot descend without the end of the splenius and complexus inserted into it coming nearer to their

other end, which rises from the vertebra ; and therefore, by his own maxim, these muscles, at least in part, serve to bring down the back-part of the head, and thereby raise the fore-part of it.

The experience he appeals to is, to apply the fingers upon the teguments covering the splenius and complexus when the mouth is opening ; when, he affirms, these muscles are not felt hard or tense, unless when the separation of the two jaws being come to the last degree, one endeavours to force further the opening of the mouth.

The answer to this experience is, That in every person, blind or seeing, ignorant or acquainted of the intention, the action of these muscle, is always felt in opening the mouth, especially when done quickly, strongly, and with repeated jerks, the person who makes the trial remembering that they are thin broad muscles covered with thick skin and fat, and with the cucullaris muscle ; so that they cannot be expected to swell so much outwards, during their contraction, as a masseter, gastrocnemius, or such other thick muscle which is little covered.

“ 4. The upper and lower jaws concur in the same manner to open the mouth, since they divide the motion which produces this effect.”

Is not this the doctrine of your essay-writer ?

“ 5. It is probably the same in the crocodile, as well as in a great number of other animals : It is known that the crocodile, in opening its throat, raises the upper jaw considerably : We are assured, in the old *memoires* of the

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"*academie*, that this elevation is greater than
"the depreffion of the lower jaw; there fix
"mufcles are described, which are in place of
"the *splenius*, *complexus*, &c. and which are
"there faid to produce the motion of the upper
"jaw."

This is such an explicate declaration of these fix muscles serving to open the throat, that I can't imagine how it came to be brought as an argument against me, unless the remark which follows is thought to explain this declaration to the reverse of its obvious meaning. The remark is, "All the muscles of which we have spoke, are only appointed to raise or bring down the head; and thus, when it is said that those which are placed upon the back-part of the back and neck serve to raise the jaw, it is not properly the jaw which is raised—but the upper jaw, and the cranium; for the bones which compose these two parts are firmly connected to each other."

In this the Academicians are guarding against their readers believing, that they joined in the common opinion which prevailed at that time, of a crocodile's upper jaw having a moveable articulation, and moving independent of the motions of the cranium, in the same way as the upper beaks of parrots move, without any intention of contradicting what they had said of the muscles analogous to the splenius and complexus raising the upper jaw.—Nay, Mr Ferrein's introducing his explication of the words of the Academicians with, *apparemment*, and afterwards with *il est a presumer, probably*, and *it is to be presumed*, shews that he was not convinced

vinced his explication was just, but rather that he gave it in complaisance to Mr Winslow.

In the second article or section of the *Mémoire* I am examining, Mr Ferrein treats of the muscles which produce the motions of the one or the other jaw; in which, to shun digressions, he premises some preliminary observations, which I shall consider in the same order he has put them.

1. (p. 519—698.) “In polygastric muscles, the contraction of one belly is independent of the contraction of another.”

I agree to this, but don't chuse to adopt all the reasoning in proof of it; which, however, not relating to the subject in dispute, I shall pass without further notice.

2. (p. 521—701.) “The inflexion of the tendon of the digastric muscle is produced by an annular ligament, and by an aponeurosis which is fixed to the *os hyoides*.”

I have described and painted the aponeurosis. Cowper * mentioned an annular ligament; and it is painted by Courcelles †, who says, “in omnibus subjectis non tam distincte conspicitur annulus;” and Mr Ferrein admits, that it is inconstant in its appearance, varying in its thickness and breadth, and being (p. 529—712.) frequently enough, and (p. 531—715.) very often, intirely wanting.

In the comment on this second observ. (p. 513—783.), there is a little mistake committed, when it is said, the 13th article of your vol. III. is accompanied with two figures of Cowper, there being

* Anat. explicat. of tab. 15. et Myot.

† Icon. Musc. capit. 4. Lugd. Bat. 1743. tab. 4.

being but one figure of Cowper's. And, immediately after, by beginning a quotation from that article in the middle of a sentence, I am made to say what I never intended, to wit, that this ring of his is nothing but a cellular membrane; whereas, by beginning the quotation five lines higher, or reading the 3d objection in art. XI. of vol. 1. every one may see, that I denied such a pulley to the tendon of the digastric muscle, as the superior oblique muscle of the eye, or the muscles of the fingers, have to play in; unless the cellular membrane had been mistaken for a pulley. Mr Ferrein agrees with me, that there is no such pulley, tho' he is pleased to give the name of annular ligament to some fibres of the aponeurosis, which, rising in greater or smaller number in some subjects, must pass over the tendon, to join others, which are extended from the inside of that tendon.—Whether do such uncertain fibres warrant one to lay down a general maxim, “That the inflexion of the tendon of the digastric muscle is produced by an annular ligament,” especially, seeing the inflexion is always made, whether there is or is not such a ligament?

The third and fourth observations in the *Memoire* being approbations of the doctrine in your essays, concerning the curvature, or tendon of the digastric, which cannot be raised without the *os hyoides*, and its want of a pulley, I shall make no remarks on the explications of them, that I may avoid more dispute than is necessary for my own defence.

Mr Ferrein, observing some fibres of the stylo-hyoid muscles joined with the digastric, concludes,

cludes, in his 5th observation, (p. 531—715.) that “the stylo hyoideus muscle, and the posterior belly of the digastric, may be considered as “one and the same muscle with two heads.”

If the connexion of the aponeurotic fibres of muscles should give the name of *digastric*, *trigastric*, &c. to all the muscles which have such connexion, the number of the many belied muscles must be greatly increased.

(P. 535—720.) It is said, “the stylo-hyoideus, “and posterior belly of the digastric, are appointed to act together, and to produce nearly “the same effects.”—How does this agree with the first observation, (p. 515—648.) “In polygastric muscles the contraction of one belly “is independent of the contraction of another?”

The 6th observ. is (p. 335—721.) “The “*os hyoides* departing from its natural situation “cannot be moved from before backwards.” The reasons for which is said to be, the ends of the cornua of the *os hyoides* then resting upon, or being supported by the tendinous parts of the *musculi recti anteriores majores* of the head, which are situated immediately upon the vertebræ.

One would think that, as muscles have a constant nifus to shorten themselves, the curve which the muscles above and below the *os hyoides* make, was intended to keep the points of its cornua from resting on the vertebræ, to prevent the parts there from being hurt by the pressure of these points. And, when one is directing no effort in these muscles, but they are left to their natural action, a finger on the base

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base of the *os hyoides* can push it back, and, when we observe the motion of this bone in deglutition we see it recede farther back than its natural situation.

The 7th observation is to confirm what I had said concerning the keeping firm or bringing down the *os hyoides* in opening the mouth. But, in the account of the dispute about the digastric muscles, the word *uniquement*, p. 537—723. and the word *seul* (p. 550—743.) ought not to have been put; for I never said that the digastric served only for deglutition.

I am here in an unlucky situation between the Academicians. Mr Winslow, as I observed formerly, blames me for considering other actions of the two heads of the digastric muscles than the simultaneous one; and here Mr Ferrein blames me as much for considering only the simultaneous one. Whoever reads your essays will find Mr Winslow has understood me best.

After what I have said concerning the impossibility of the condyles of the lower jaw ever being the fulcra or axis of the head in its motions, it is needness to shew Mr Ferrein's misapplication of his observation of the maxillary condyles advancing forwards in opening the mouth, which (p. 538—725.) he would have us to believe is the head shuffling back. He will see the mistake by only trying whether he feels this shuffling backwards in the mastoid process, and whether the ligaments which join the occipital condyles to the vertebræ are long enough to allow this shuffling back of the head?

The use of the external pterygoid muscles, in bringing forwards the condyles and meniscoid cartilages

cartilages of the lower jaw, is next described in nearly the same manner as in your essays; and (p. 539—727.) the same action of bringing down the lower jaw is ascribed to the mylo and genio-hyoidei muscles; but, in proof of this last proposition, I am made to say, that the *os hyoides* moves towards the lower jaw, when the jaw is brought down; or at least I must be inferred to say so from my asserting, that the muscles above and below the *os hyoides*, having a considerable curve at this bone, must draw it forwards when they are acting.

If I had not said that the *os hyoides* is brought down and restrained from coming too far forwards in opening the mouth, there might have been some grounds for the inference above mentioned; but, as I affirm both those facts, the inference ought not to have been made from one circumstance, without mentioning the other two.

The second proposition (p. 540—729.) is, “The stylo-hyoideus muscle serves to raise the upper jaw;” the proof of which is the motion of the two jaws upon the same axis; the motion of the upper jaw very different from the natural motion of the head, and altogether independent of the contraction of the splenius, complexus, or its other elevators; and therefore, the raising of the upper jaw must be by the action of the stylo-hyoideus and posterior head of the digastric.

If your readers are convinced, that the circumstances in the antecedent proposition are disproved, as I have endeavoured to do, they will reject the consequence here drawn.

Tho’ we follow Mr Ferrein in his supposition
(p. 541

(p. 541—729) of the stylo-hyoideus muscle being extended to the chin instead of being fixed to the *os hyoides*, we would not find this muscle could make any effort to raise the upper jaw: For the head always performing its motions on the spine, and the styloid process, which is the part of the head to which this muscle is fixed, being farther forward than the axis of the head's motion, the muscle never can draw down the back-part of the head to raise the fore-part.

The 1st proof the stylo-hyoideus raising the upper jaw is founded on the following general rule: "Every muscle being connected at its two ends to two different bones, all motion of one or the other of these bones, which causes one of these ends to come nearer to the other end, depends, at least in part, on the contraction of that muscle. The rule, it is added, is perhaps new."

Is it new to you, gentlemen, that muscles shorten, or endeavour to shorten themselves, when they act, and that they must be shorter when their ends come nearer each other? And, if the rule is true, does it not prove what I formerly inferred from it concerning the action of the splenius and complexus?

Whoever lays it down as an universal principle, that every muscle becomes shorter when it acts, or that every muscle which shortens itself, is exerting its force, is in a mistake, as is evident in numerous examples. Thus, when one sits down on a seat, the extensor muscles of the legs, and the retractors of the thighs are in strong action, yet are lengthened, while the flexors

flexors of the legs and thighs are shortened, but unactive — The extensors of the spine are the acting but elongated muscles, when we bow forwards. Nay, of two muscles which seem to have nearly the same direction and use, the one may act without the other. Thus the *biceps flexor cubiti* and *brachialis internus*, generally act together in bending the fore-arm; but, if the fore-arm is to remain extended, or is to be kept fixed in any degree of flexion, while the arm is to be strongly raised and drawn forwards, the biceps contracts strongly and the brachialis is unactive; or if the fore-arm is to perform strongly the action of pronation, while it is to be bended with little force, the brachialis may act while the biceps is unactive, lest this latter muscle counteract the pronators, whose force is then principally required.

I hope the feeble crispation of the muscles which I have said to be unactive in the foregoing cases will not be called their action, in order to find a contradiction in my words. Every one surely knows the difference between the weak natural curtation of muscles and their strong voluntary contraction.

If the rule is faulty, the example of the stylohyoideus, as here applied to illustrate it, is one of the most unfavourable in the body, because, this muscle being fixed to a part of the head farther forward than its axis of motion, it is impossible such a muscle ever can raise the head.—But, supposing the connection of this muscle to the head to be farther back than its axis and supposing the cornua of the *os hyoides* to rest with their

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their points on the vertebræ of the neck, can it be imagined that such slippery round points could possibly stand so firm on the slippery vertebræ as to be a fulcrum to the head in its elevation, especially too while the traction is at the other end of the cornua, and in a very oblique direction? or could such points be pressed with so much force upon the muscles covering the vertebræ without hurting them?

In my answer to Mr Winslow, I mentioned what I thought the use of the stylo-hyoideus muscle in opening the mouth; and therefore may pass Mr Ferrein's second proposition, which treats of that subject, without examination, as I may also do to his third proposition, wherein he assigns the same use to the *mylo, genio, sterno, coraco hyoidei* muscles as I had done.

In his fourth proposition, (p. 543—732.) he undertakes to prove "that the mylo and genio-hyoidei assist in certain cases the stylo-hyoidei to raise the upper jaw, but that the stylo-hyoidei never can assist the former to bring down the lower jaw."

The power of the stylo-hoidei to raise the upper jaw, and the cornua of the *os hyoides* serving as fulcra to the head in its elevation, being the foundations on which the proof of this proposition is laid, I need not do more than mention it, and refer to what has been said against these foundations of it, which may also serve as an answer to what is said of the posterior head of the digastric muscle (p. 544—545—734—736.) where the same principles are made use of.

What is said (p. 544—545—734—735.) of the anterior head of the digastric, is appealing to his
general

general principle about the shortning of muscles, and repeating what Mr Winslow had said, which, being already examined, need not now be considered.

Had not Messrs Winslow and Ferrein been so fond of the impossible reversion of the head, upon the condyles of the lower jaw, they might have had a better argument than any they have employed for the action of the digastric muscle in opening the mouth: They might have observed, that the posterior belly of the digastric is fixed to the skull, at least as far back as the condyles of the occiput, where the axis of the motion of the head is; and therefore, when that muscle acted, it might draw down the back-part of the head, and so raise the fore-part, while the anterior belly brought the lower jaw down.

Lest some other make use of this argument against me, I must observe, that the axis of the head is much farther back than its center of gravity, on which account the head falls forward on the breast, whenever the organs that support it are weak and unactive, as in infants, sleeping, drunken, fainting, apoplectic people, whenever they are put in the erect posture. The organs which support the head from falling forwards are the splenius, complexus, and other muscles placed on the back-part of the neck, as is evident from the head's bending forward when these muscles are cut transversely, from the retraction of the head when they are convulsed or inflamed; and many more such proofs might be named. These muscles are not only strong by their number of fibres, but they are inserted in-

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to the head in a much more advantageous way, than most muscles of the body are in their respective bones; for they are fixed into the head nearly at right angles, and at a considerable distance from the axis, that they may have a long lever to act with; and their other end is fixed to the vertebræ, where little motion in the direction of their traction can be allowed. Whereas the digastric muscles have few fibres, are fixed very near the axis, at a very acute angle, and are connected at their other end to a very loose moveable part; and therefore can never officiate instead of the other powerful organs. Nor would the fixing of the *os hyoides* by the muscles below it render the digastric any way equal to the office of raising the head, while the action of the posterior head of the digastric would diminish the force by which the lower jaw is drawn down just as much as the power of its own action: For it must be evident that the lower jaw can be drawn down with no greater force by muscles fixed to the *os hyoides*, than what is exerted to keep that bone down. Whenever the power is greater that draws the *os hyoides* up, than what pulls it down, it must be moved towards the jaw, which it never is in opening the mouth.

P. 547. 548—739 It is granted, according to what is in your essays, p. 117. that the mylo-and genio-hyoidei muscles draw the jaw backwards.

I pass several little things which may be criticized in the four following pages, to come to the conclusion of this *memoire*; where it is asserted, "That Bidloo, Cowper, and Monro conclude it impossible to swallow, while the mouth is open;

“ pen ; which reasoning is destroyed by experience, as may be observed in those who drink, pouring the liquor from above, into the mouth.”

I can see no such assertion in Bidloo, either where, in *tab. xv.* of his *Anat. corp. hum.* he has painted the digastric muscle, nor where the lower jaw is delineated in *tab. xcii.* nor is it to be expected any where in his large anatomy, because, in the preface it, he declares, he is not to give the history of the uses of the parts, nor have I observed it in his *exercitationes*.

Cowper, in the explic. of Bidloo's *tab. xv.* and in both the editions of his *Myotomy*, says, “ When we swallow our aliment, we shut our mouth,” which is true ; but he mentions nothing of the impossibility of doing otherwise.

Monro's words are, “ *Essays vol. i. p. 111.* ; ” One can scarce swallow any with his mouth open, which might be translated into French, “ C'est avec beaucoup de difficulté qu'on peut avaler quand la bouche est ouverte,” which I suppose Mr Ferrein will allow.—The experiment of swallowing when liquor is poured into the mouth from above, does not prove it not difficult to swallow with the mouth open ; and in this situation, the weight of the liquor supplies, in a great measure, the office of the digastric muscles.

The challenge of the accuracy of quotations, puts me in mind to remark, that Mr Platner, whose pen is said (*p. 427—578.*) to have been employed about the use of one of the muscles of the lower jaw, does no more than relate some of the things which authors, and particularly

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Mr Winslow and I had said, declaring, *p. 14.*
Nec in has lites ire, nec eas componere ausim.
 He will neither enter into, nor determine this
 disputed question.

If any of your readers think I have wrote
 more, and you have published more than the
 subject in dispute deserves, they will, I hope, be
 so good as to remember, that, on my part, self-
 defence is allowable, and the example of the
 Academy of sciences, who have inserted 92
 pages in 4to, on this subject, into their memoirs,
 may plead your apology. I might perhaps have
 omitted all the things in which Mr Ferrein a-
 grees with me; but I began to fancy they were
 of value enough to deserve to be repeated, be-
 cause this ingenious gentleman generally intro-
 duces them with *j'ay éprouvé, reconnu, trouvé,*
montré, or some such phrase of approbation.

Answer to Professor WALTHER.

IN a dissertation *de Oscitatione*, published at
 Leipfick in 1738, the professor, *p. 12.* criti-
 cizes my fourth objection to the office, com-
 monly ascribed to the digastric muscles, in your
 vol. 1. art. xi. The character which a very
 good judge * gives of this gentleman's wri-
 tings, obliges me to transcribe his own words,
 that others may know whether I do him justice.

After giving a translation of my objection, he
 says, “*Itaque, ut luculenter adpareat, quomodo*
 “*ille scriptor, hac doctrinæ suæ parte, motus*
 “*componat, qui aliter a se distingui debent, hinc*
 “*ita arguendo rem declaramus: Caput recl-*
 Y 2 “*nare*

* Haller. Method. stud. Medic. p. 549. *distione paulum
 obscuriore usus est.*

" nare eundem affectum habet ac si in vivo
 " homine id extendatur. Hinc idem erit, atque
 " maxillam inferiorem ab inferiore, præsertim in
 " cadavere, ubi hæc per se stupida cernitur, dis-
 " cedere jubere, et ita os aperire. Porro, caput
 " reclinare, idem erit, ac digastrici originis lo-
 " cum demittere, et, si manfores musculi maxil-
 " lam in vivis fixerint, hanc capitis reclinatio-
 " nem, contractione digastrici juvare; sed præ-
 " ter hunc casum, eundem musculum debilitare,
 " et pro maxilla deducenda inertem reddere;
 " nihilo minus tamen eundem, tanquam si liga-
 " mentum esset, longiorem facere, et ita inten-
 " dere, simul, os hyoides non propria, naturali,
 " ac facillima contractione musculi, sed plurium
 " partium communi tensione ac motu, levare.
 " Nam omni occasione, os ipsum quoque tolli-
 " tur, etiamsi maxillæ committantur, et tantum
 " caput reclinetur; sicut, quod inter pomum
 " Adami et os hyoides spatium est, ac illo tem-
 " pore admodum augetur, etiam in vivis hanc
 " rem probavit. Eadem fallacia communis at-
 " que proprii motus adsertio nititur, cum cl.
 " Monro præsumit; toties os hyoides deorsum
 " duci, quoties maxilla vivi hominis descendit.
 " Verum, ubi caput erigitur, et binæ maxillæ
 " committuntur, quin et deglutitione accidit,
 " ut priores hyoidei musculi et pars stylo-pharyn-
 " gæi os bicornæ aliquantum tollant, aut alium
 " membranarum nexum hoc os sequatur, ut, di-
 " gito altero ad mediam basin, et altero ad cornu
 " admoto, facile percipitur. Sed, quando jam
 " maxilla movetur, tum hoc fere levissimo mo-
 " do os hyoides demittitur: imo, eo tempore,
 " sterno-hyoideos aut sterno thyroideos musculos
 " non

" non contrahi, vel exinde judicare Monro potu-
 " isset, quod hic maxillæ motus, et oris hiatus,
 " motus proportionem, quo os hyoides descendit,
 " creberrime superet; et maxime, quod, ore
 " clauso, perinde ac aperto, scutiformis cartila-
 " go, sub osse hyoideo, in alterutrum latus, pa-
 " ri facilitate converti possit."

The objection here, I imagine, is, that the
os hyoides rises when the head is extended, and
 descends when the head is bended, though the
 mouth is shut all the time; the rising of the
os hyoides in the former case being owing to the
 stretching of the parts, without any contraction
 of the muscles which are connected to the *os hy-*
oides, as the descent of this bone in the flexion
 of the head depends on the natural elasticity or
 restitution of the overstretched parts stretched
 beyond their tone.

If I had proposed no more to be done, than to
 observe what happened to the *os hyoides*, when
 the head was alternately bended and extended,
 the objection would have been reasonable; but,
 as I desired muscles to be drawn with hooks and
 threads, while the posture of the head remained
 the same, and appealed to the feeling of mus-
 cles hard, swelled, and tense, while the differ-
 ent actions were performed by a living person,
 this objection can have no force.

It is to be wished too, we had been here told,
 how a muscle becomes weaker and unfit to do its
 office, by stretching it, and making it tense, as
 seems to be affirmed in regard to that digastric
 muscle; for, on the contrary, it is a doctrine
 generally received, that muscles exert most force
 when they are tense.

The particular objection to the sterno-hyoidei,
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and sterno-thyroidei muscles acting in opening the mouth, to wit, that very frequently in this action, the lower jaw moves proportionally more, and the mouth is wider opened than the *os hyoides* descends, will not be insisted on by any who consider, 1. That, supposing these muscles to be the only depressors of the jaw, the chin would move downwards much more than the space which these muscles contract, or than the *os hyoides* descends. 2. That muscles placed between the *os hyoides* and chin, contribute to the opening of their mouth by the contraction, the quantity of which is not so great as the defect of the chin, occasioned by this very contraction, is.

To illustrate these propositions, and some others mentioned in my reply to the two learned academicians, and to show some other things not generally attended to, in considering the motions of the jaw, allow me to make use of a little figure, representing these parts in their motions.

Let A, fig 3. be the axis of motion of the lower jaw.—B, C, D, the chin. AB then is the jaw in the reclined or extended state of the head. AC is the jaw in the middle or erect situation. AD is the jaw fully depressed.—Let E be the *os hyoides*. EB a muscle fixed to the *os hyoides* and chin. EC, ED, the same muscle shortened as the jaw descends. ED being in the same plane with the jaw, and EB perpendicular to that plane.—Let EF be a muscle extended between the sternum and *os hyoides*.

Hence it appears, 1. That the depressors of the jaw being fixed at a greater distance from the center of motion, than most other muscles are fixed to their respective bones, have a longer lever to act with.

2. Supposing

AND OBSERVATIONS. 259

2. Supposing the *os hyoides* E unmoved, while BE acts, the space which EB contracts, is so much less than the chin descends as Bg, the difference between BE and CE, is less than the space between B and C.

3. Less shortening of the muscle EB is required as the chin descends, to make the chin move equal spaces, for Ch or gi, is less than Bg, tho' the space between B and C and between C and D are equal.

4. The angle ABE, is larger than ACE, or any angle which could be between the muscle and the chin, moving from B to D; and, the farther EB recedes from being perpendicular to AD, the angle between the bone and muscle always becomes less; that is the angle of insertion of this muscle is decreasing as it continues to act.

5. If ever the muscle and jaw are in the same plane, as AE and ED here coincide, the muscle may draw the bone back, but can no longer depress it; and, if the bone was brought lower down by any other power, this muscle would raise it, instead of drawing it down.

6. The muscle EF acting may be considered as fixed to the point E, *e*, *e*, of the jaw; and therefore this muscle shortens itself so much less than the chin moves, as the spaces between *e* and *e*, or *e* and E are less than the spaces between B and C or C and D.

7. If the *os hyoides* E moves downwards at the same time that the jaw descends, then the muscle EB requires to have a less quantity of contraction, than when E is fixed, to move the jaw thro' the same space; from C to *k*, is longer

longer than EC, and the angle of insertion does not decrease so much, the angle ACk would be larger than ACE.

8. If E moves only forwards while the jaw descends, the muscle EB requires to shorten itself more than when E remains fixed. A line drawn from C to l, is shorter than CE, but the angle of insertion does not decrease so much; ACI is larger than ACE, and EF acquires a longer lever.

9. When the *os hyoides* is moved forwards and downwards while the jaw descends, the angle of insertion of EB decreases less than when it is fixed, and EF acquires a longer lever; but the quantity of contraction of EB may be more or less, or the same, according to the proportion of the advancement forwards and of the depression of E.

XIV. *An Account of a Child born with the Urinary and Genital Organs preternaturally formed; by Mr JAMES MOWAT Surgeon at Langholm.*

IN November 1732, a child was brought forth, whose *funis umbilicalis* was tied to the upper edge of a deep hole, at the place marked C, (see tab. II. fig. 3. where all the parts are represented of the size they now are of; and just above the *ossa pubis* this deep hole penetrated the peritoneum; but now a lump of spongy flesh D rises out of it. The edges of the hole were at first and are now found. From the spongy flesh of this hole arise two papillæ A, B, about the size of the point of an ordinary probe: At which

Fig. 1.

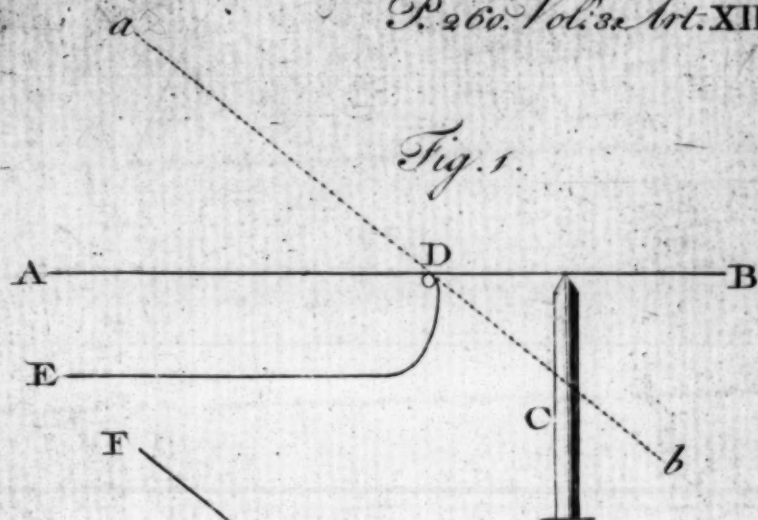


Fig. 2.

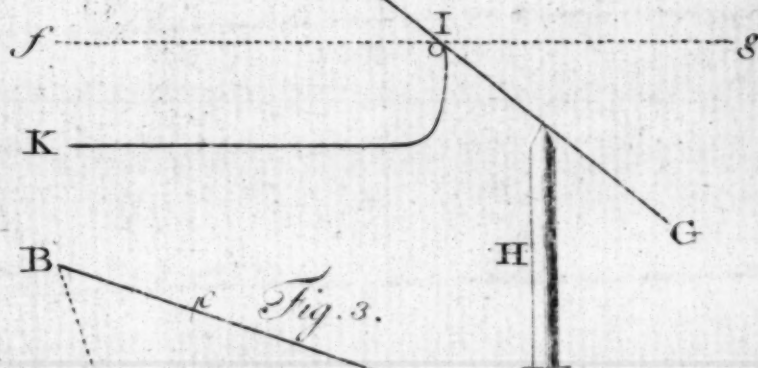
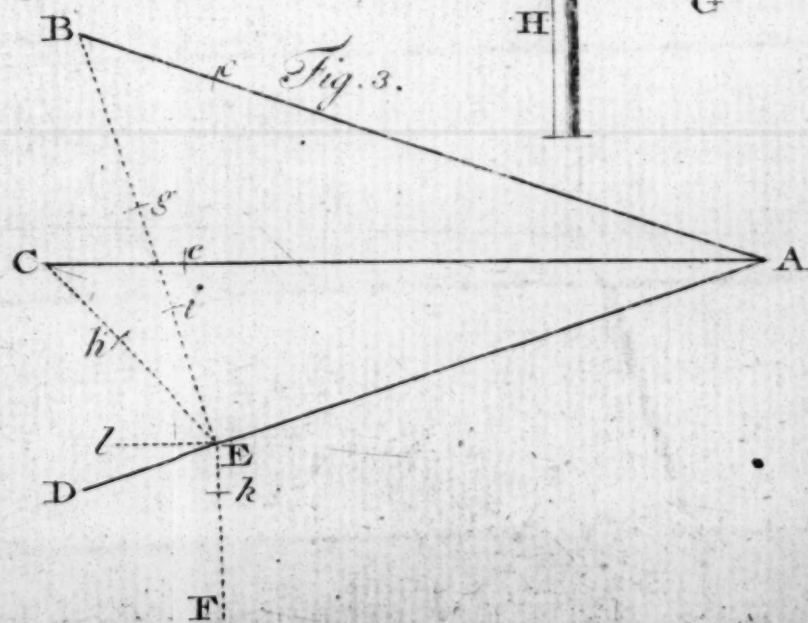


Fig. 3.



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which the urine continually ouzes; but, when the child cries, the urine jets out as blood from the aperture of a small artery. The papilla on the left side at A is now closed, but the other at B keeps open as at first, through which the urine continually ouzes, and, scalding all the parts, keeps them very raw.

The penis arises immediately at the under side of the hole, is now of the same bulk with the figure G, but was at first much less; it was and still is imperforated, and flat upon the upper side next the glans, as if it had been slit longitudinally; It has two small blue veins marked *a, a*, and a large prepuce inverted at H.

The scrotum and testes were and continue in a good condition; the scrotum KK is corrugated very close to the end of two prominencies marked E, E; the raphe, I, appears in its middle with the wrinkles on each side; the testes, F, F, lie under the prominencies, they can be moved from the place they are seated in, either higher or lower as we please to put them.

The distance between the scrotum and anus is longer than it should be; and the ossa pubis are longer and flatter than in other children.

There is a large prominency, E, E, on each inguen, under which the testes lie.

The child is healthy, active, and stirring, and is very much affected with his misfortune; for he frequently cries upon viewing himself, and is very unwilling any other should see these parts.

The mother is a very healthy woman, has brought

brought forth another child every way sound and right in all its members. She tells that in May, before she was brought to bed of this her first child, she was struck in the belly with a cow's horn; she recovered the hurt in two or three days, but the fright remained longer with her, and did terrify her sometimes in her sleep.

I have twice or thrice thrust down a small silver probe at the right papilla B, about an inch or more, but cannot feel it in the perineum. I have also closed up the papilla two hours and three quarters with a small tent and an astringent plaister over it, but can discover no swelling in the perineum, but rather in the belly. Upon withdrawing the tent, the urine squirted a great way; but by the uneasiness of the child, and the anxiety of the mother, I'm obliged to desist at present from any further experiments.

XV. *An Essay on the Diseases of the Lacrymal Canals; by ALEXANDER MONRO, Professor of Anatomy in the University of Edinburgh.*

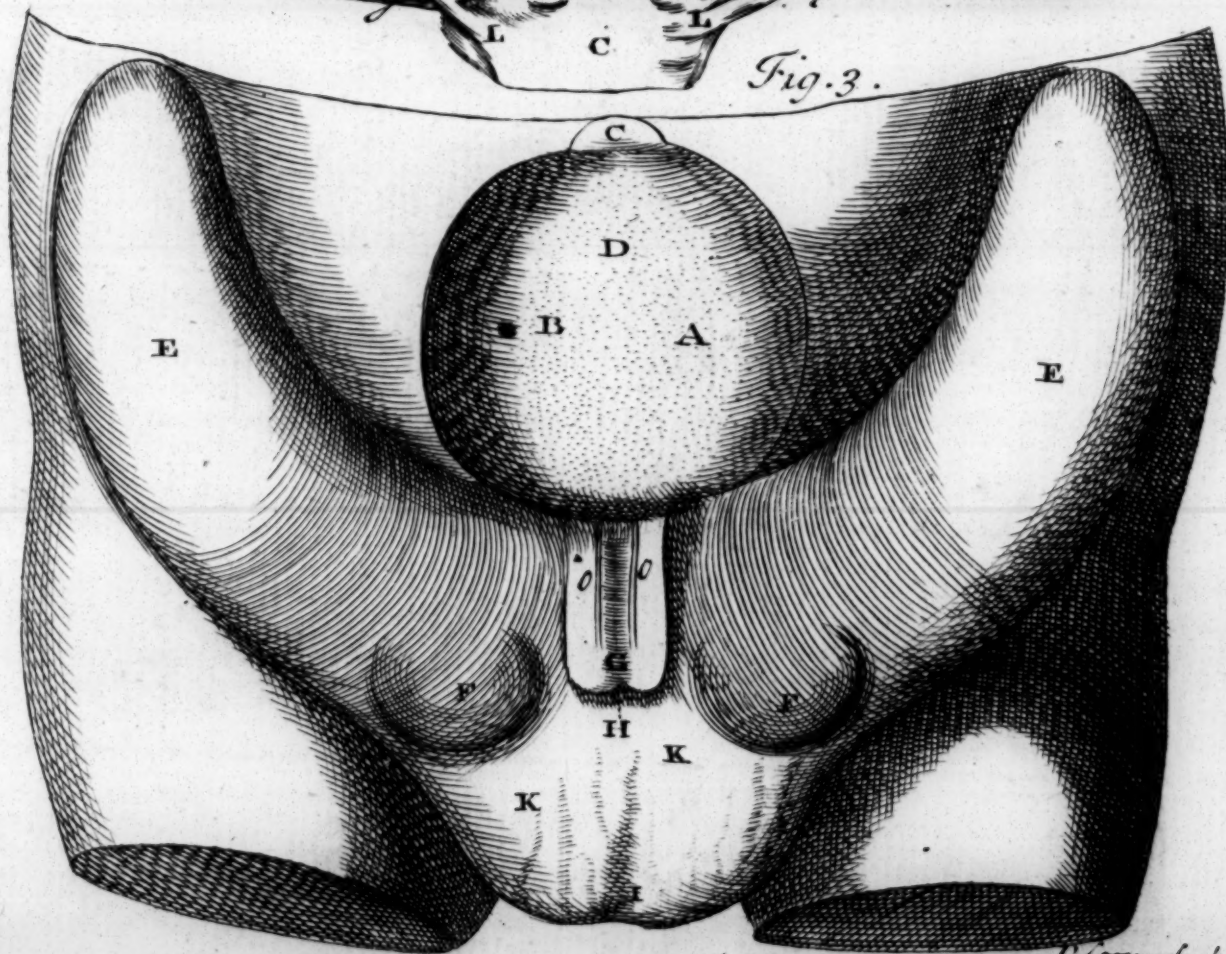
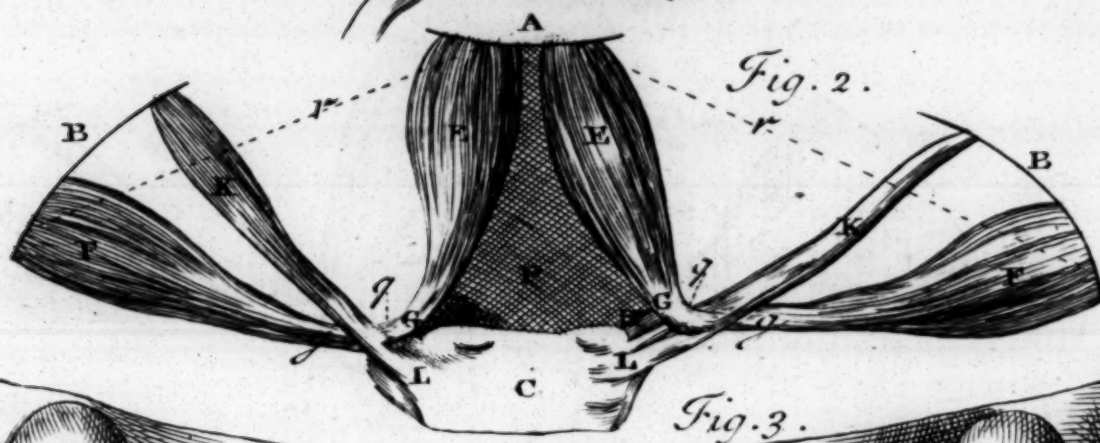
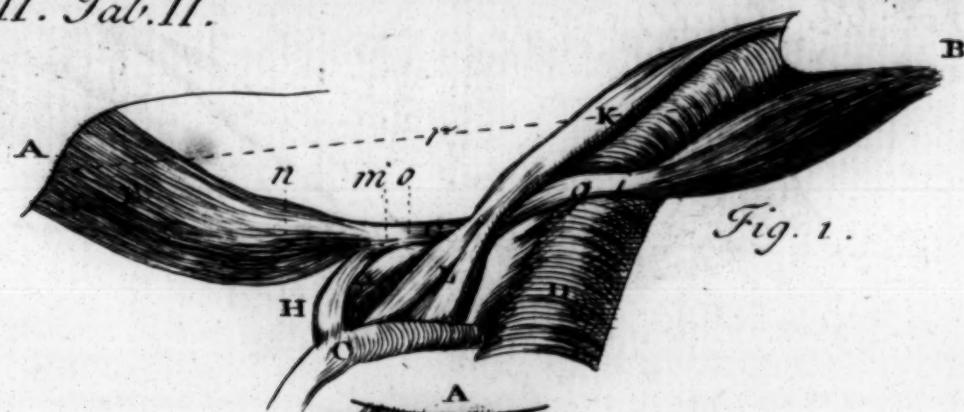
THE many improvements that have been made of late in most chirurgical operations, as they shew how imperfect surgery was formerly, so they should be an incitement for endeavouring to improve it still further; which will be found no difficult task to any who carefully considers the natural structure and situation of the parts that are affected in the several external

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nal diseases; who examines the changes which these diseases do, or may make on the body; who from thence lays down reasonable intentions of cure, whereby the parts may be brought as near to a natural state as possible, or the functions of such as are disordered or destroyed may be supplied by art; and lastly, who diligently weighs the manner, immediate effects, and consequences of every step to be taken in executing these intentions. I shall endeavour to shew, in the following remarks on the *fistula lachrymalis*, how far the want of due attention to these necessary circumstances is capable of keeping us in ignorance, and leading us into error. I made choice of the *fistula lacrymalis* for an example, because it is a common enough disease that has been often seen and treated by surgeons, is wrote of in all the systems of surgery and in most collections of observations, and is more particularly examined by the professed oculists; yet, in my opinion, is very little understood, and has very defective or faulty rules laid down for its cure.

I shall not trouble you with critical observations on the old distinction of anchylops and ægylops, or on the impropriety of reckoning all ægylopes or ulcers of the internal canthus of the eye to be *fistula lacrymales*, or on the characters of any ulcer necessary to constitute a fistula; but shall only inform you, that the disease I now treat of, is such an indisposition of the canals that convey the tears from the eye to the nose, as does not allow the tears to pass as they ought: If you think the name of *fistula lacrymalis* does not agree to this description,
you'll

you'll do a favour in changing it for a more proper one, or in assigning names to the different cases I shall suppose.

The lacrymal canals, whose diseases are the subject of this essay, having been described by several anatomists, but, none of them having these diseases in view, their words or pictures will not probably give your readers an idea of these parts suited to my purpose; wherefore I shall give a short description of them, illustrated by the figures that are sent with this paper, before I enter upon any account of their diseases. (See tab. III.)

The two lachrymal points, A, B, (Fig. I.) situated each on a little prominence near the internal extremity of the edge of each palpebra, take in the tears to be conveyed by two little ducts of about four tenths of an inch long, which are continued from the points inwards and somewhat downwards, (the superior being the longest and most oblique), till they open into the lacrymal sac, D. Between the points and the angle where the palpebræ join, the *caruncula lacrymalis*, C, is placed. The lacrymal sac, D, lies upon the groove formed partly in the nasal process of the maxillary bone, and partly in the anterior half of the *os unguis*, to which its back part adheres slightly; but this membranous bag is connected firmly to the ridge which is raised on the *os unguis*, at the posterior part of the groove, serving at this place as a proper distinguishing boundary between the orbit and exterior parts; so that the lacrymal half of the *os unguis* is without the orbit, while its posterior half constitutes a share of the bony sides of that cavity.

Such

Such another firm connexion of the lacrymal sac to the bones is also to be observed at the fore part of the groove, where a small future joins the *os unguis* to the nasal process of the maxillary bone.

The lacrymal groove of the *os unguis*, D, is about two tenths of an inch broad in its middle widest part, and is about half an inch long from the top, till it is covered by the maxillary bone, and a compleat bony canal is formed for inclosing the whole lacrymal duct; which, after a short progress, opens into the nose immediately below the middle of the superior edge of the lower *os spongiosum*, where its extremity becomes smaller than any other part of it.

When we view the side of the nose after the bones have been divided by a perpendicular section, we see the *ossa spongiosa*, K, L, fig. 2., situated near horizontally, depending by their superior edge from the other bones, and removing farther from them as they descend. The anterior extremity of the superior *os spongiosum* K, is fixed to the other bones very near where the upper part of the *os unguis* is joined to the frontal bone; and the superior edge of the inferior, L, is very little below where the great lacrymal duct begins.

This short description will, I hope, assist your readers to understand the several morbid cases I am now to consider.

If, after any erosion of the eye-lids, the lacrymal points, or the small pipes going from them to the lacrymal sac, are intirely blocked up by their sides growing together, which may be known by the constant weeping of the affected

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eye,

eye, after a disease capable of producing such an erosion, without any tumor, but on the contrary with a depression of the teguments covering the lacrymal sac, and by the points being so obliterated, that one of Anel's small probes cannot be pushed by them into the sac; in such a case, I say, the patient must all his life bear the deformity and uneasiness of a weeping eye, or some such operation as the following must be attempted: Let the lacrymal sac be opened in a slow cautious dissecting manner; after which push a small round curve needle with a waxed thread from one of the prominencies of the palpebræ, where the lacrymal point naturally is, into the superior part of the sac; draw out the needle at the aperture lately made, and leave the thread by way of a seton; do the same at the part where the other *punctum lacrymale* was. Soon after the small inflammation these threads may raise is over, the briny tears trickling along them will make the passages callous and fit for supplying the office of the natural ducts, when the threads are to be taken out; and the aperture in the sac, which has been kept open by dossils, and refreshing sometimes with the lunar caustic. will very readily shut up as soon as this manner of dressing is forbore, and that it is only covered with a pledgit. The success with which an artificial passage, formed this way into the mouth, has supplied the salivary duct (See *art. XIII. vol. 2.*) may make us judge, that the method just now proposed might also be successful.

When the fibres of the lacrymal sac are too weak, or the large duct is obstructed by some concrement

concreted liquors, the sac is gradually stretched by the tears which regurgitate frequently at the *puncta lacrymalia*. Some call this disease a dropsy, others would have named it a hernia of the lacrymal sac. We know it by the tumor of the sac without hardness, discolouring, or pain, which disappears as soon as we press out the tears at the *puncta lacrymalia*. While this disease is recent, these tears are pure, afterwards some pus appears with them, because of the excoriation which the sac suffers. Lest there should be any hazard of mistaking a tumor or small abscess in the teguments, which cover the lacrymal sac, for the disease of the sac just now described, as I have seen done, allow me to mention, that such tumor is easily distinguished from the hernia or dropsy, by its not diminishing or not discharging a large quantity of tears or pus at the *puncta lacrymalia* upon pressure.

The method of cure in the dropsy, is to pass one of Anel's probes from the *puncta lacrymalia* into the nose, to remove any grumous matter that may be lodged in the lacrymal canals, and then to inject, by the lacrymal points, mild, detergent, and gently astringent liquors; such as mel-rose diluted in lime-water, to which a little brandy may afterwards be added, or any chalybeat water, or a little weak wine, &c. which injection is to be repeated twice or thrice a-day; and in the intervals the cure is assisted by external compression, made with compress and bandage, or the proper compressing machine, and by corroborants. We read of several cures performed in this manner by Mr

Anel and Mr Heister; I have also had success with it.

It will be necessary here to observe, that the small duct, going from the superior lacrymal point, A, fig. 1. being more oblique than the inferior, it will be more proper for passing the probe by; and, because the passage from that point into the nose is not straight, the probe must be bended into a small arch of a large circle. The superior eye-lid being then raised, and its edge turned a little outwards with the fingers of one hand, the surgeon, resting the other hand on the patient's cheek near the exterior canthus of the eye, introduces the probe, with its convexity upwards, into the punctum, and raising his hand gradually as he pushes the probe forward, he brings it almost perpendicular to the lacrymal sac, by the time that the probe reaches the lower part of that sac; then he turns the probe softly, till he brings its concavity towards the nose; and pushes it downwards thro' the great duct into the nose. After which he moves it up and down, and to different sides, to break any concremented matter lodged in it.

The inferior *punctum lacrymale*, B, will be fitter for introducing the pipe of the small syringe into, and for making the injections by, because the inferior eye-lid has not near so much motion as the superior, and is more easily held with its edge turned a little outwards, at the same time that the surgeon has a better rest on the patient's cheek for the hand that holds the syringe, than he can have in making the injection by the superior point.

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A proposal has been lately made to introduce a small pipe at the lower part of the lacrymal duct, into the sac, by which medicines may be conveyed for curing several diseases of the lacrymal canals ; but I don't know whether cures have been made with this pipe.

If these methods of injecting and compression are not successful, and the internal part of the lacrymal sac is become spongy and ulcerated, which we judge to be the case, by the quantity of pus expressed with the tears, the sac must be opened by an incision. When the sac is much distended, and the matter in it is thick, it is as easily opened as any common abscess ; but, when the swelling is little, and the matter is thin, I have observed, that, notwithstanding the skin was kept as tense as I could betwixt my thumb and two fingers, and an assistant endeavoured all he could by pressure on the *puncta lacrymalia*, to prevent the tears and pus escaping by these orifices ; I say, notwithstanding these precautions, I observed that the pressure of my knife squeezed out the liquors contained in the sac, and made it collapse so much, that it could not be opened without a manifest risk of cutting the posterior side of the sac, and so laying the bone bare, which evidently appears necessary to be shunned in the case I now speak of. When the sac is full of spongy flesh, or has contracted after an unsuccessful attempt of the operation, it is scarce possible to know when the sac is opened, or how much of it is opened, by an incision, without having some directory for the knife. To avoid these inconveniencies, in such cases, I introduced a small probe at one of the *puncta lacrymalia*,

lacrymalia, and caused an assistant to raise up the sac with it, while, with a gently-crooked sharp pointed bistory, I cut the stretched teguments in the common way, till I felt or perceived the naked probe; when, laying aside the bistory, and taking a pair of crooked scissars, I introduced the probe-pointed blade into the sac, and cut it first upwards, and then downwards, till its whole length was opened.

In making this opening, the tendon of the orbicular muscle of the eye-lid must be cut through; but it is of no consequence, for the firm cicatrice afterwards ties that muscle to the bones here sufficiently, to prevent any inconvenience. We are however to take particular care not to cut so near to the joining of the palpebræ, as to be in any hazard of dividing them, which might occasion a considerable deformity; and it will be more convenient to save the angular artery and vein, than to wound them; because, if they are wounded, the blood which they pour out hinders the operator to see so distinctly what he is doing.

After the sac is fully opened, we can observe in what condition it is and are at liberty to free the nasal duct of any thickened matter that happens to be in it. Small dossils, armed with some proper medicines, either of the detergent, drying, or strengthening kind (according to the morbid state of the sac) are laid into the sac, but without being made very hard, or being stuffed strongly in, lest unnecessary pain and inflammation should be occasioned. The lips are then covered with a small pledgit, and this is kept on by a semilunar snip of adhesive plaster;

ster; and, if the dressings do not keep well enough in the sac, compresses, and the compressing instrument, should be put upon them. While the disease is curing by proper medicines, the lips are kept fresh with the lunar caustic. When once the sac is made sound, the orifice in the teguments closes very soon after the use of the dossils is forbore, if the patient is in any thing of a tolerable habit of body. I have practised this method with success.

When the large *lacrymal duct* is excoriated, or has fungous flesh rising from it, which will be known by the acute pain, or great insensibility, and by the difficulty of passing a probe through it after the sac is opened, and by a view of its superior part, there is a necessity of dropping or injecting proper medicines into it, and of keeping its sides from becoming contiguous, by introducing some convenient substance into it. When medicines are made to pass through it, the patient must be desired to hold his head forwards, that the liquors may run out at his nose, instead of falling back into his fauces. In my opinion a small tent of lint, secured with a thread, and armed with medicines, is preferable to a small wax bougie, or any thing that is oily, because these keep the parts raw much longer; and do not imbibe suitable medicines. As soon as the duct is brought to a right condition, the disease is the same as in the preceeding supposition.

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Let us now suppose, that the fungous flesh rising from the sides of the duct, has united and blocked up the passage entirely, which can only be discovered after the sac is open, by the impossibility of introducing a probe, or making liquors pass through it, into the nose, while it feels soft and yielding, without that sort of firm resistance which a bone covered with a membrane makes, and which cannot be described well in words, but what all surgeons of any practice know. In the case as I have now put it, I think there is still no necessity of hurting the bones, in order to make a passage for the tears. What I would propose is, to push a small shoemaker's awl, or some such instrument, through the middle of the fungus into the nose, and then to keep this artificial passage open, and to render it callos by a tent or seton. In making the perforation, the instrument must be held with its concavity towards the nose, and it must be thrust through slowly, and with no great force, the surgeon changing its direction a little when ever he touches the bone with its point; when the drops of blood coming out of the nose, shew the instrument to have perforated far enough, it is drawn back, and the tent or seton must be immediately passed in the same way. The seton is preferable in my opinion, but requires the probe by which it is introduced to be of very flexible silver, and previously brought to a particular form. You see, *tab. III. fig. 3.* a probe about three inches long, bended into a semicircle, only with near half an inch toward the point pretty straight; this I have made to pass from the lacrymal sac
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into the nose, and brought it out at the nostrils of several dead bodies, without using force, or changing its form. The small cord that is brought through in the eye of this probe, is to remain for some days, without shifting the part engaged in the duct, till it becomes loose by the suppuration which happens round it; then it may be drawn a little, having besmeared the part that is to be next introduced with some gentle suppurative balsam. In a very little time the suppuration will waste as much as is sufficient; and then the detergents and desiccatives will succeed in bringing the canal near to the natural state. I need scarce mention the tying of the two ends of the cord after each dressing, to keep them from hanging over the face, whereby they would be in hazard of being unwarily pulled, or the necessity of dressing the sac all the while, as in the former supposition; or taking out the cord and curing all up, after the duct and sac are found, they are so obvious.

If the duct has been blocked up in a child, and no cure is attempted till the person comes of age, the duct may be so obliterated or small, that the method now proposed cannot be executed, and an artificial passage must necessarily be made through the bone: But as this case can never be certainly discovered till the sac is opened, we ought still to proceed in the cautious way I formerly mentioned for this part of the operation. The place of the *os unguis* where this perforation ought to be made, will easily be determined from the description I gave of
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the parts, and from the advantage of the canal for the tears, being at the most depending part of the sac. It must not however be attempted to be made where the natural duct was, for, in piercing the bones in that place, and with that direction, the instrument will more readily pierce into the large *sinus maxillaris*, than into the nose. If the perforation is attempted to be made too far forwards, the instrument must be forced through the maxillary bone, which cannot be pierced but with great difficulty. I imagine any one may easily understand the place and direction for making the perforation right, by observing how the two pins are placed in *fig. 1.* and how they come through the *ossa spongiosa* in *fig. 2.* at M and N; for the pin E, in *fig. 1.* being thrust perpendicularly through the *os unguis*, about the middle of the lacrymal sac, pierces the anterior extremity of the upper *os spongiosum* at M, in *fig. 2.* and the pin, F, in *fig. 1.* thrust very obliquely through the *os unguis*, at the lowest part of the sac, pierces the *os spongiosum inferius* at N, in *fig. 2.* The pin, F, placed so far back as to be contiguous to the ridge dividing the *os unguis* with the perpendicular direction of the other pin, E, would answer all intentions without any risk.

The instruments, with which this perforation has hitherto been ordered to be made, appear to me very faulty. One general fault to all of them is, their destroying more of the *os unguis* than is necessary or safe; for, where-ever the orbital part of it is diseased, there is great danger of an inflammation and suppuration be-
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ing brought on the muscles and fat within the orbit, which may be attended with a train of troublesome dangerous symptoms, that art can do little to relieve, because of the quantity of fat, in which pus diffuses itself easily, and cannot be restrained by medicines or compression in such a cavity as the orbit, and in the neighbourhood of such a sensible moveable necessary organ as the eye. The actual cautery gives great pain, burns the neighbouring parts, raises inflammation, and leaves a carious piece of bone to exfoliate, which retards the cure much. The directory, or blunt stilet, when pushed through the bone, fractures it far and near, and often rushes into the nose so far as to break the *septum narium*. The olive-shaped blunt perforative, or the tapering strong forceps, make large fractures in the bone, besides opening a passage large enough to let the point of one's finger pass, where one no larger than a crow-quill is required. In place of all these then, I would propose always to make use of a drill, small perforative of a trepan, gimblet, trocar, or any such small instrument that can perforate with little force and no fracture. The trocar is the instrument I have made this perforation with several years past, in the following manner. Chusing a trocar the stilet of which is of the size I wish to have the perforation, and is as much longer than its cannula as I think necessary for perforating into the nose, I put a probe into the cannula, to search for the lowest part of the ridge of the *os unguis*, at the fore-part of which I keep the probe till the cannula is pressed

pressed softly down to that part of the bone. When the probe is taken out, the cannula is put in the proper direction, and the stile of the trocar is then put through the cannula, to be worked with half-turns of the hand till it perforates into the nose, in doing which its own weight is sufficient, scarce is any pressure of the hand required.

So soon as the perforation is made, withdraw the stile, to pass a tent secured with a thread, or a piece of wire-drawn lead, through the cannula into the nose; then, taking away the cannula, accommodate the tent or lead to the rest of the wound; or the cannula may be taken away with the stile after the perforation, and the tent or lead, or a little pipe, may be put into the hole, having the head of the tent broad, or the lead split a little at its outer end, that each part may be bended down to make a head, and the pipe should have little shoulders to prevent it from falling into the nose: Then dress the lacrymal sac in the same way as was proposed above, when we supposed the sac to be laid open. Allow these dressings to remain till the suppuration comes on, when they are to be renewed. Whenever the inflammation is gone, by drying medicines injected at the new orifice, or conveyed into it by the tent, endeavour to harden the membrane with which the thin edges of the perforated bone soon cover. I used mel-rose and a little brandy, increasing gradually the proportion of this last medicine. Whenever the tent can be made to pass this hole, without giving pain, leave off the use of the tent or pipe, and cure up the external

external orifice, as soon as it will go together, which is very soon, if its lips have been gently touched from time to time with the lunar caustic. In this way I have cured those who had this disease from their infancy, without one bit of bone exfoliating, or the least weeping in the eye afterwards, or other inconvenience, not so much as an observable scar.

I am informed that a gold pipe has sometimes been left in the new passage; the cut fore-part of the sac having united, while it remained there.

Let us now suppose, that the sharp matter in the lacrymal sac has destroyed its membrane, and rendered the *os unguis*, on which it lies, carious, or that the caries having begun in the bone, the ichor of it has eroded the membrane. In this case, if there is a large passage eroded also thro' the *membrana narium*, while the teguments are whole, it may be long before the lacrymal canals can be discovered to be affected; and the disease will be treated as an *ozæna*. But, if there is no such passage into the nose, the malady may be known by the brown-coloured stinking ichor diluted with tears, which may be squeezed out at the lacrymal points, upon pressing the lacrymal sac.

The method of cure here will be to open the lacrymal sac, as in the former cases mentioned, to separate as much of the bone as is carious, to make a perforation with the point of a lancet or bistory, through the *membrana narium*, and then, to complete the cure, as directed in

the preceeding supposition of the bone being artificially perforated.

The separation of the carious bone is ordered to be hastened by the application of the actual cautery, tinctures of myrrh and aloes, and of euphorbium: But, in my opinion, the breaking away with a pair of forceps all that is carious will be much more speedy, and is not attended with such inconveniencies as the other methods are.

You must have observed, that I have hitherto supposed the diseases of the lacrymal canals to be attended with no opening of the teguments made by erosion, nor with any maladies of the neighbouring parts; and, I believe, you will see there is no necessity of insisting at any length upon them. For, when there is an opening in the teguments, near the internal canthus of the eye, we can easily discover whether the lacrymal canals are affected, by pressing pus out of the *puncta lacrymalia*, before the ulcer is cleaned; and, after the pus is wiped away, the tears will run out at the external orifice; which also gives a better opportunity of introducing instruments to discover the state of the diseased parts.

In the cure there is nothing different from what has been formerly directed, unless that the opening into the sac is more easily made, where the external orifice is large enough to allow the necessary instruments to be introduced; and, when it is too small for this purpose, we must enlarge it, by putting into it tents of sponge made firm and hard, by being soaked in some melted

melted plaster, and then kept pressed under a weight, or in a press, till the plaster hardens; or this sponge-tent may be prepared, by simply wetting the sponge in water, or a diluted mucilage or glew, and then rolling pack-thread firmly round it, and hanging it up thus till it dry.

There is such a great variety of diseases which may accompany these maladies of the lacrymal canals, whether as causes, consequences, or accidental attendants, that it would be to engage in almost a system of physic and surgery to give a detail of them; and therefore I shall pass them without any further examination.

XVI. *A Tumor of the Nose unsuccessfully extirpated; by—*

THE author of art. XXII. in your first volume, having had the benefit of your promise to conceal the names of those who send you unsuccessful cases, I claim the performance of the same promise in the publication of this paper, if you think it deserves a place in your collection.

A child was born with a small moveable tumor on its nose, which increased as the child grew, otherwise the child was healthy and strong, having only had a short fever or two, and passed some worms, before five years of age, when my advice was first asked concerning that tumor, which now was so large as to cover all the nose, except the nostrils, and was so prominent

minent to each side, that the eyes were in part covered with it. Towards the base, it was so soft, that, by pressing a finger on each side, they felt each other, but at the most prominent part there were several hard round knots. When I saw it, the child complained of no pain, tho' I was informed that sharp lancinating pains sometimes struck through the tumor. I held the nostrils, while the child forced its breath that way, but saw not the tumor rise any. I searched into the nostrils with a probe, but could neither feel any excrescence, nor push the tumor or teguments outwards. The *os frontis* was firm, and united in the middle. From all which I concluded the bones of the nose to be compleat, and therefore was of opinion the tumor (which would make the patient very miserable by increasing, and would bring death at last) might be safely extirpated. Being, however, taught by *Job à Meekren* *, and some others, how deceitful excrescences of the head, brought to the world with a child, might possibly be, I would not undertake the cure, till another surgeon, of more experience and longer standing, in my neighbourhood, who justly has a considerable character, should examine the tumor, and assist me in whatever was determined to be done. That gentleman joining in opinion with me, I undertook the extirpation with his assistance. When I had dissected about half the base of the tumor off, I observed the bones of the nose to be incompleat; and that the membrane of the nose, part of which I had laid bare,

* Observ. cap. 7.

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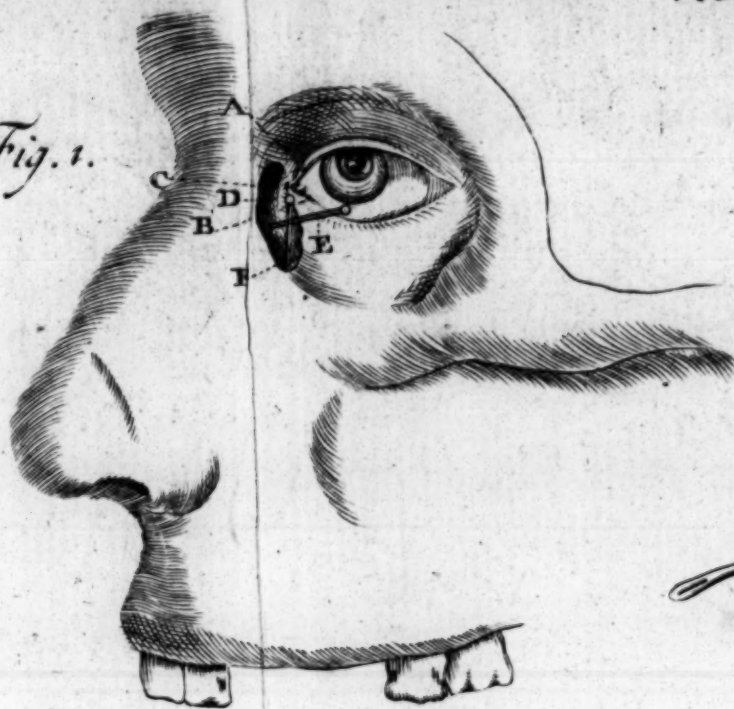


Fig. 2.

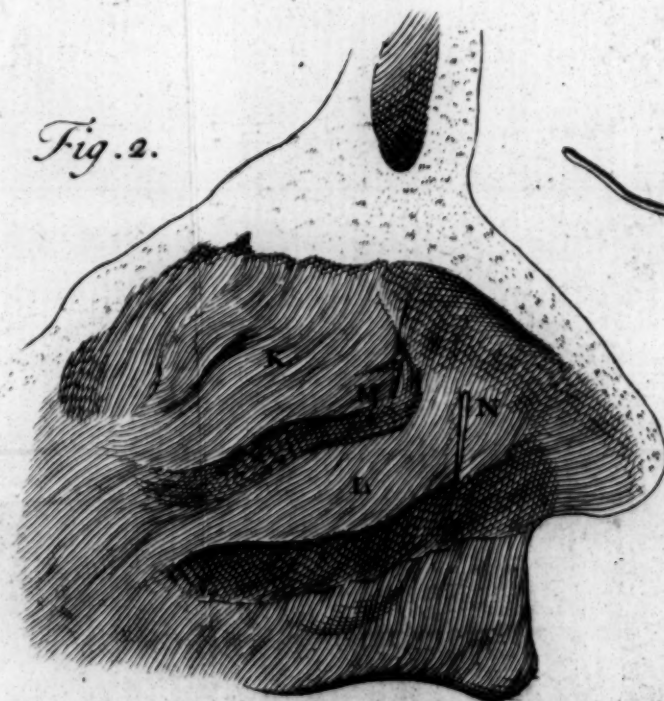


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bare, was moved outwards in expiration, and inwards in inspiration. Not being certain how far upwards the bones might be wanting, I dissected all the tumor off at the lower part, but left a little of its base above. Having stopped the bleeding, I dressed the wound in the common way.

When the tumor was examined, it appeared all of a substance little firmer than the common fat under the skin, except where the knots were, which were of a schirrous hardness.

The child passed the first night pretty easily. Next morning the pulse was a little quick, attended with a thirst, and a sickness at the stomach, which had made the patient vomit once. An emollient clyster being injected, and emulsion given for drink, these symptoms abated. Towards the evening the dressings seemed moister than they commonly are so soon after a wound.

In the morning of the second day after the operation, the dressings, child's hair, and head-cloaths, and the pillow under its head, were all wet with a watery liquor, which had a particular smell that I never felt in any wound before, and do not know how to describe. The dressings being taken off, we saw that this liquor ouzed fast from the bared membrane of the nose, though we could not perceive the orifice by which it escaped. We applied from time to time, *bol. armen. pulv. helvet.* chalk, sugar of lead, white vitriol, burnt allum, blue vitriol, quick-lime, brandy, alcohol. oil of turpentine, spirit of nitre dulcified, plain spirit of nitre,

oil of vitriol, lunar caustic, the actual cautery. In short, we applied every thing we could think of that had any chance for stopping this ouzing of lymph, but without success. On the sixth day, our patient vomited a long round worm; in some time after fell into convulsions, and in an hour more died.

XVII. *An Account of a Procidencia Uteri; by*
ALEX. MONRO, *Professor of Anatomy in the*
University of Edinburgh.

—Scobie being seized with a fever, which continued some days, in the month of August 1728, when she was not full three years old, had a considerable discharge of blood by the vagina for three days; after which she seemed to be in perfect good health about twenty days, then complained of pains in her belly, loins, and thighs, and had such another evacuation. The quantity of blood voided was judged by her mother to be as large as what she herself commonly had in her menses. The child suffered regularly such returns every three weeks, or, at furthest, within the month, attended with the same symptoms, without any considerable loss of strength, or decay of her body, till the month of May 1729. But, during the third monthly evacuation, which was at the end of September 1728, her mother observed a small swelling rising out from the orifice of the vagina, which disappeared as soon as the hæmorrhage ceased. This tumor, however, came out larger at each period thereafter; but, upon the
child's

child's being kept in bed three or four days, and the flux of blood stopping, always disappeared, till May that it came out of a considerable bulk, and did not return as usual. From this time there were no more periodical evacuations of blood; but, instead of these, there was a perpetual dropping of a white mucus from a hole in the lower part of the tumor; which mucus was sometimes in so large quantities, that, if a swath had been applied some hours about it, to prevent that liquor from coming away in drops, as frequently was done, whenever the swath was taken off, the mucus was thrown out so abundantly, and with such force, as made those present to imagine it was urine which the child passed.

About the end of July, the parents having brought the child to the hall of the college of physicians, where Dr John Riddle and Dr William Porterfield were then attending to give advice to the poor; these two gentlemen having viewed the child, desired the parents to carry her to me.

Being informed of the preceeding history by the child's mother, I examined the parts, and found a tumor, G, (see tab. IV.) hanging out at the vagina, as big as a hand-ball, the neck of which, F, was about an inch diameter. At the lowest part, H, the tumor was largest, and of a faint leadish colour: Behind the most prominent part of it, I discovered a hole of $\frac{1}{4}$ inch diameter, by which I introduced a probe, I, some inches; and then the probe was resisted, and the child complained of pain. From this hole, there

there was a constant stillicidium of mucus. Round this orifice the tumor felt hard and firm; but a little higher, where it was largest, it was softer, seeming to be composed of a cellular substance; at this place scales had frequently formed and fallen off. The neck, F, of the tumor was very smooth, of a shining red colour, and very solid and hard; I introduced a probe betwixt this neck and the sides of the vagina, two inches upwards, and turned it all round the circumference of the neck. The clitoris, D, nymphæ, B, B, and orifice of the urethra, E, were natural enough, only the neck of the tumor pressing on the urethra occasioned some difficulty in the excretion of urine; and the urine being diffused over the *labia magna*, A, A, and other neighbouring parts, by striking against the large base of the tumor, had somewhat excoriated these parts.

The child could scarce sit, and straddled when she walked, but lying a-bed she was very easy. Her complexion was pale, and her body small, otherwise she was healthy. Having consulted with the two gentlemen who had sent her to me, and several other physicians having seen her, the disease was unanimously judged to be a *procidentia uteri*. Wherefore I attempted to reduce it; but the tumor was so large and firm, I could not accomplish it. Fomentations and cataplasms, first of the emollient and discutient kind, were applied; afterwards they were formed entirely of the attenuants, and lastly astringents were tried. In the mean time the child underwent the general evacuations as much as her strength could bear, without the tumor's yielding

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ing in the least, but, on the contrary, daily increasing; at last she began to turn hectic, and the tumor to be disposed to gangrene on its outer surface, which were in vain endeavoured to be prevented by diet and antiseptic medicines. I several times considered of the amputation; but, being sensible of the body of the uterus being to be cut through, and frightened by the ill success Ruysch and some others had in this operation, I had not courage enough to undertake it. The child in the mean time turned weaker, the tumor gangrened in its external surface, and, by the gangrened parts falling off, it was reduced to near half its former bulk. Ten days after which, (7th November), she died.

On opening the abdomen next day, the bladder K, was full of urine, the left ureter, M, was in a natural state, but the right one, N, was distended, by urine, to four times its natural diameter, and the kidney from which it came was larger, softer, and paler than the other, but without any appearance of the folliculi or vesicles sometimes found in morbid kidneys. The urine had certainly been retained in the bladder by the neck of the preternatural proci-dentia pressing on the urethra, and the distension of the right ureter was owing to a steatomatous body, U, some more than an inch long, and seven tenths of an inch broad, which lay behind the ovarium and *ligamentum latum*, and reached to the cervix of the bladder, to which it firmly adhered, and through its exterior extremity the ureter passed.

There

There was scarce any thing of the uterus to be seen, till the bladder was reclined over to one side, when a small part of its fundus, O, appeared.

The *tuba fallopiana*, Q, Q, were near perpendicular to the uterus and the ovaria, T, T, were situated contiguous to them.

Having made these remarks, and carefully observed the situation in which the several parts were, I dissected off the peritonæum and its cellular membrane from the bones and muscles composing the sides of the pelvis, and brought away all the parts contained in that cavity with the right kidney and ureter; and then, that a view of the whole might be had in one figure, I dissected the left side of the bladder away from the peritonæum, and reclined it over to the right; after which, having, with a needle, passed threads through the skin where the *mons veneris* and external or great *labia pudendorum* are, I gently stretched the skin of these parts, and secured it in that posture by help of the threads which were tied to a probe, and two pins which I had made fast to the table; in which posture Mr Cowper delineated it, having his view obliquely from the left side and from above. The figure he drew, and afterwards graved, will, I believe, better explain the situation and connection of all the parts, than any description, and is the only one I know that gives a distinct idea of this disease my patient laboured under, a true genuine *procidentia uteri* covered with the vagina, and without any inversion of the womb.

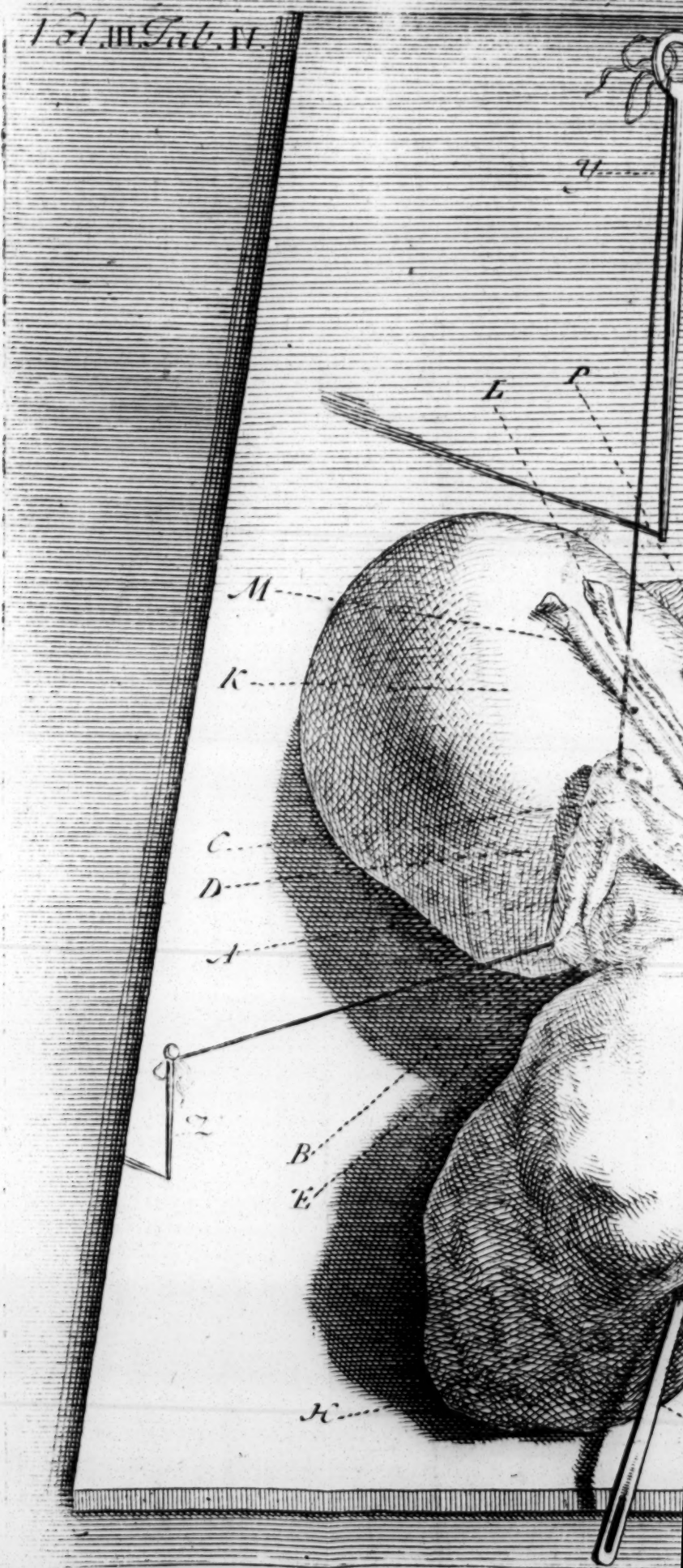
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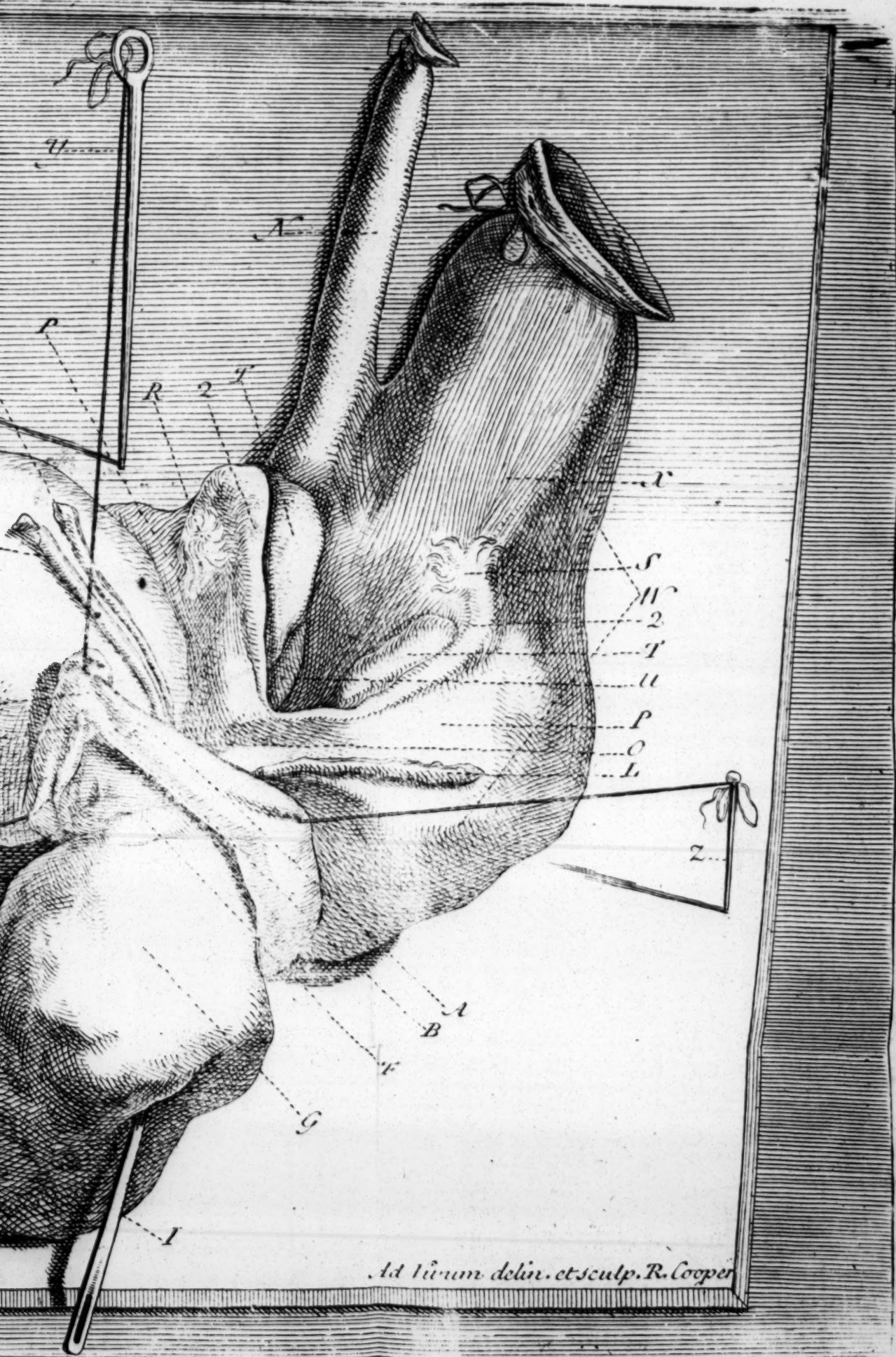
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Tab. III. Tab. II.





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AND OBSERVATIONS. 287

- A. A. The two great *labia pudendorum*.
- B. B. The nymphæ.
- C. *Præputium clitoridis*.
- D. *Glans clitoridis*.
- E. The orifice of the urethra.
- F. The neck of the procidentia as it came out at the vagina.
- G. The left side of the tumor, which was much diminished by the falling off of the gangrenous parts.
- H. The right side which had no parts cast off.
- I. A probe put into the uterus by its internal orifice.
- K. The bladder distended with urine, and reclined over to the right side.
- L. L. The jagged edges of the peritonæum both on the left side of the bladder, and of the pelvis where it was cut, to remove the bladder to a side.
- M. The left ureter of the natural size.
- N. The right ureter greatly enlarged with urine.
- O. The *fundus uteri*.
- P. P. The *ligamenta lata*.
- Q. Q. The *tubæ Fallopiæ*.
- R. The fimbriæ of the right tuba with its orifice in view.
- S. The left *morsus diaboli* seen on the side averse to the orifice.
- T. T. The ovaria.
- U. The extremity of the steatom appearing from under the right ovarium.
- W. The thick *tunica cellulosa* at the side and back part of the pelvis.
- X. The *intestinum rectum*.
- Y. The

Y. The probe to which the thread supporting the *mons veneris* was tied.

Z. Z. The pins to which the threads stretching the great labia were fastened.

After the figure was drawn, I endeavoured to discover by dissection, how far the inverted vagina or uterus had each been increased in their bulk to form such a large tumor; but they were so intimately united, that I could not distinguish the substance of the one from that of the other; and therefore could not determine their proportional thickness.

XVIII. *The Description of a Pessary, invented by THOMAS SIMSON, M. D. Professor of Medicine in the University of St Andrew's.*

THERE is no calamity that afflicts the fair sex more than the *procidentia uteri*, when they labour under it; for it is accompanied with perpetual uneasiness through their whole body: It gives the greatest hindrance in following out the common affairs of life, and frequently ends in ulcerous and cancrus tumors.

The causes of this disease are many; but, which ever takes place, there is no hope of a cure, unless the part is kept in its natural situation; for its weight, when it hangs unsupported, does more harm than there can be service done by any medicines applied for strengthening and bracing the relaxed fibres; and therefore the first step towards a cure of *procidentia*

procidentia has always been to reduce the uterus to its natural situation, and to keep it there by means of the instruments named *peffaries*.

All the peffaries which I have seen described by authors, or used by practisers in medicine, seem to me defective and inconvenient; for they answer the design by their bulk only, and must be thrust violently into the vagina, which not only occasions a great deal of uneasiness and pain to the patient, but a skilful hand is always required to manage them; which many women are unwilling to allow. To evite the many disadvantages attending the use of the common peffaries, I contrived the one delivered with this paper, which, I can assure you, after sufficient experience of it in such cases, has exactly answered all the intentions without any inconvenience.

Fig. 1. of tab. V. represents the exterior side of one of the plates of tin, of which the instrument consists.

A, is the body of it, the sides of which, B, B, C, have holes made in them for sewing a piece of oiled leather on it.

D, a narrow neck in the form of half a hinge going out from the large plate.

E, a round plate full of holes, for sewing upon it a hemisphere of cork boiled in oil.

Fig. 2. shews the form of the other plate with its anterior side exposed to view, and with the leather and hemisphere of cork sewed to it.

A, the plate at the sides of which, B, B, C, the edges, appear turned in, with the threads

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which secure the leather on its outside crossing over them.

D, a spring of japanned steel, which is fastened to the plate at E, but stands out from it at the other extremity.

F, the convex side of a hemisphere of cork, sewed to such a plate as E represents in Fig. 1.

Fig. 3. is the figure of the instrument mounted.

A, B, the two large plates.

C, the spring keeping them at a distance.

D, one of the small round plates with the threads, by which the hemisphere of cork is fastened to it.

E, E, the two hemispheres of cork.

F, waxed threads made to cross from one hemisphere of cork to the other, and left of such a length as to allow the spring full play, in separating the plates and corks.

G, G, two skains of waxed thread passed through the ends of each plate, and secured from being drawn out by the knots at their extremities.

Having seen D, the half of a hinge in fig. 1. one can easily imagine what could not be seen in this picture, viz. such another applied to the side of it, when these necks of the two plates are made to cross, and that a small axis being put through them, they move easily.

When this instrument is to be introduced, the two plates are pressed close together, and the sphere of cork is put as high up into the vagina as is convenient, taking care to have the flat sides of the plates towards the right
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and left of the vagina. Whenever the fingers pressing the plates are removed, the spring pushes the plates and hemispheres away from each other, to press only on the sides of the vagina, without any danger of straitening the urethra or rectum. The cross threads now extended between the hemispheres, hinder the uterus or plicæ of the vagina to fall down between the corks, so as to be in danger of being bruised, when the sides of the instrument are again pressed together, in order to take it out; and at the same time liquors will pass freely. The corks and plates covered with leather, when pressed against the vagina by a spring, which needs not be very strong, cannot bruise its coats much; and their being oiled preserves them from corrupting soon, as the metal of the plates and japanning of the spring prevent any bad consequences from rust.

When this instrument is to be taken out, the plates are pressed together; or, if it has been wholly lodged within the vagina, it is brought away by drawing the skains of thread. And the patient can with little or no trouble, introduce or remove this instrument at pleasure.

XIX. *An Account of the sides of the Os Uteri being grown together in a Woman with Child; by the same.*

A Woman, forty years of age, observably narrow between the *ossa pubis* and the *os sacrum*, had been four days in severe labour of her first child, when I was called to assist

her: The child appearing to have been dead for some time, I opened its head, and extracted it, but with great difficulty, its shoulders and haunches being too large to pass in the straitened passage between the bones. During some days after her delivery, she passed a great many small rugged stones by the urethra, and at length, after her urine had been stopped some time, her husband drew out of the urethra a large piece of thick membranous substance, three inches in length, and in some parts two inches broad. One side of it was covered with a crust of small sharp stones, the other side was inflamed and bloody; which made me judge it to be part of the coats of the bladder separated; and I was confirmed in this opinion, by introducing a catheter into the bladder; for, whenever it touched certain parts of the sides of the bladder, blood came with the urine. The patient continued a long time with a plentiful suppuration about the pudenda; but we did not suspect that the pus came from the internal parts, but only from the exterior, which had been somewhat lacerated.

About three months after her delivery, she fell again with child, and took her pains after the ordinary period. She continued two days in hard labour before I saw her. The midwife then informed me, that the inner orifice had yielded nothing; I left her half a day, and things remaining in the same way at my return, I examined her condition, and found that the *os tinæ* had not only not yielded, but that the sides of it were grown together, without any vestige of a passage; whereupon

I asked the assistance of another physician, and Dr Haddow being called, was, as well as the midwife, sensible of the case being such as I judged it to be.. Wherefore we agreed to make an incision into the *os uteri*, but were first obliged to dilate the vagina sufficiently, that we might operate more securely. We had no *speculum matricis*, and therefore behoved to supply it by some other instruments. We tried to make the dilatation with a pair of long broad-bladed forceps, but they neither had strength to dilate sufficiently, nor did they keep the vagina equally open. After this we caused two pieces of wood, each three inches long, and two and a half broad, to be made concave on one side, and convex on the other, and of no more thickness than we thought would be sufficient to bear a strong enough pressure by the necessary dilatation. When these were finely polished, and besmeared with grease, I introduced them into the vagina, with the concave faces towards each other; then sliding in the legs of a *speculum oris* between them, and turning its screw, I separated the pieces of wood so far as we could see distinctly the cicatrice of the grown-together parts, and could have easy access to divide them, which I did by an incision, at least half an inch deep, before I pierced thro' the substance of this part of the womb; then, immediately introducing my finger at this wound, I touched the head of the child, and felt the whole circumference of the passage hard like a cartilage, which yielded nothing to several throws she had after the incision: So that I was obliged to guide a narrow-bladed

scalpel with my finger, to make several incisions into this cartilaginous ring. In doing this, there was not the least appearance of blood, and the patient had no trouble, except what the dilatation of the vagina gave her. The labour continuing, the passage dilated a little, but not so much as to give any hopes of its allowing the child's head to pass, notwithstanding the bones of the cranium were overlapped; and, therefore, I was obliged to bring away this child as I had done the former. In this birth, there was no liquid with the child, nor did any blood follow it; it was quite supple, and had a white chalky crust over its whole body; so that we were convinced it had been dead some time.

The want of waters was some surprize, till I recollected, that, in the time of labour, she told us they were passing, at which time I had the curiosity to make a strick observation, and found, that what she called the waters passed by the urethra, which opened externally by three different orifices: This, with her having lost such a portion of the bladder formerly, and her being subject to the gravel, gave me ground to think there was some communication between these passages and the cavity of the womb, above the *os tinæ*, which had allowed the waters to be evacuated. I was the more inclined to entertain this supposition, because frequent instances have been observed of stones making their way through the neighbouring parts, as happened lately to a boy in this neighbourhood, who passed a very large stone, which had lodged long in the bladder by the anus, by which the
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urine had its course for some time after.

My patient, immediately after being put to bed, was seized with a pleuritic pain, very high fever, and difficult breathing; which coming on so soon after her being fatigued several days with hard labour, during which she slept none, but drank much of every thing in the way, appeared to me rather the cause of her death, in twenty-four hours after, than any consequence of the incisions I had made; for she never complained of uneasiness in the parts I had cut, nor had any hæmorrhage. Notwithstanding all the sollicitations I could use with her relations, I could not prevail with them to allow me to open her body.

XX. *The Description of a Forceps for extracting Children by the head, when lodged low in the Pelvis of the Mother; by Mr ALEX. BUTTER Surgeon in Edinburgh.*

THE forceps for taking hold of a child's head, when it is fallen so far down among the bones of the Pelvis, that it cannot be pushed back again into the uterus, to be extracted by the feet, and when it seems to make no advances to the birth by the throws of the mother, is scarce known in this country, though Mr Chapman tells us it was long made use of by Dr Chamberlane, who kept the form of it a secret, as Mr Chapman also does. I believed, therefore, that a sight of such an instrument, which I had from Mr Dusè who practises midwifery

wifery at Paris, and who believes it to be his own invention, would not be unacceptable to you; and the publication of a picture of it may be of use to some of your readers.

Tab. V. Fig. 4. represents this instrument seen obliquely, of one third of its real dimensions.

A, is the extremities of the blades made more concave in the middle, than is necessary to fit them to the surface of the convex head of the child, in order, as Mr. Dusè said, to hinder them to compress the temporal arteries.

B, is the convex side of the one blade.

C, the convex side of the other.

D, the hinge where the two blades cross.

E, a large flat button of a screw, which serves as an axis to the hinge, and can be taken out at pleasure.

F, a second hinge, by which the blades can be joined when the child is higher up than can be conveniently reached by the instrument, when the other hinge is employed.

G, G, the handles.

When this instrument is to be used, the axis of the hinge is to be taken out, and each blade, being directed by one hand in the vagina, is to be introduced separately along the side of the vagina, and betwixt it and the side of the child's head, as far as immediately above the ears; then, the two blades of the instrument being crossed, the axis is put into the hinge, which the operator finds most convenient to employ; after which the child's head is to be taken firm hold of, and the operator pulling by the handles, extracts the child.

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I think Mr Chapman is in the right to desire the axis not to be put in ; for it is very troublesome to take out and put in again, when any of the blades quit their hold, and the instrument can easily be managed without it, in extracting the child in the manner mentioned ; and, in several cases where it may be requisite to dilate the lowest part of the passage at the same time that the extraction is making, the blades of the forceps require to be separated, and are not to be crossed or moved upon a hinge.

You will easily see, that often when the head of a child is a little too far forward on the *ossa pubis*, or turned too far backwards, that one blade only of this forceps can be employed to bring it to a right situation, and to assist the birth.

XXI. *An Account of a malignant Lues Venerea, communicated by Suction, in the City of Cork, 1728 ; by EDWARD BARRY, M. D. F. R. S.*

THE Venereal Disease, in the common way of infection, has been fatal to many. In this view, it demands as much attention as any disorder. But, if an accident of this nature, which I now transmit to you, is not guarded against, it may become a more general misfortune, and to such who least deserve it.

A woman in this city, who was commonly employed to draw the breasts of lying-in women, had probably, in the course of her business,

ness, received the infection in her mouth; which she either did not suspect, or concealed, till she had communicated the poison to several persons of distinction.

I think this infection was still more remarkable for its malignity, and the quick progress of the symptoms, than for the uncommon manner of its being received. As I had an opportunity of seeing most of the unhappy persons who were thus affected, I thought that a faithful account of its appearance, and the method of cure, which I found successful, might not be unworthy of a place in your essays.

The nipple first became lightly inflamed, which soon produced an excoriation, with a discharge of a thin liquor; from thence red spreading pustules were dispersed round it, and gradually spread over the breast, and, where the poison remained uncorrected, produced ulcers. The pudenda soon after became inflamed, with a violent itching, which terminated in chancres, that were attended with only a small discharge; and, in a short time after, pustules were spread over the whole body. It finished this course, with all these symptoms, in most persons, in the space of three months.

This disorder made a quick and dangerous progress in such who first received it; they not being apt to suspect an infection of this nature in their circumstances. The husbands of several had chancres, which quickly communicated the poison, and produced ulcers in the mouth, and red spreading pustules on the body: But such of them escaped who had timely notice of
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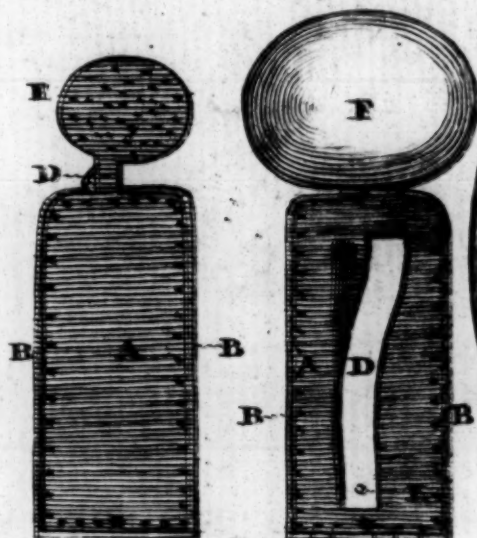


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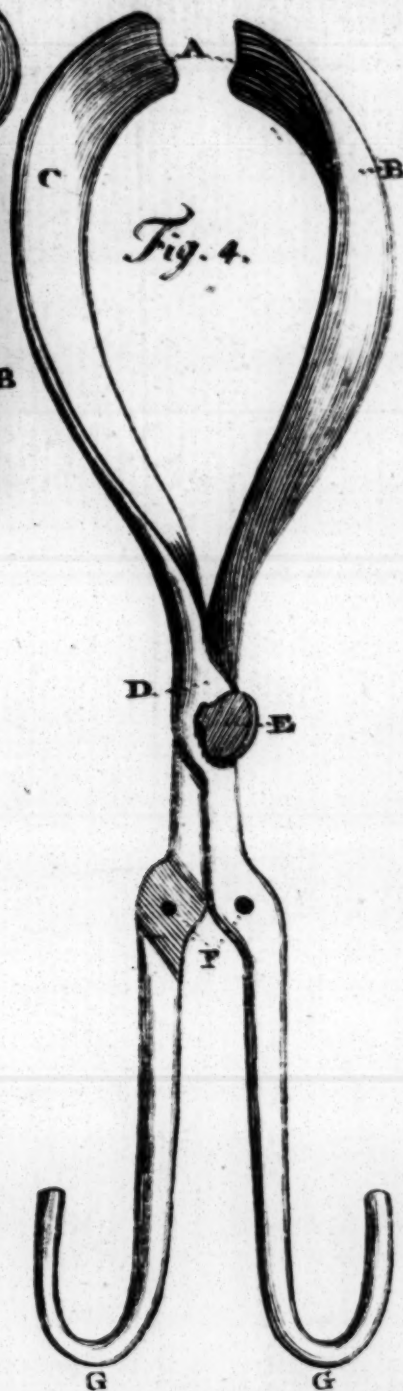


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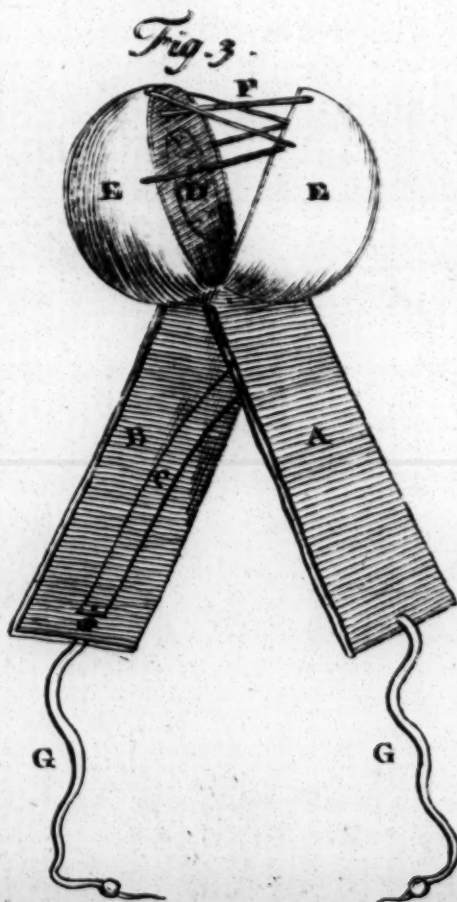


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the nature of the disease, before the pudenda were affected. Some infants received it from their mothers, and to the greatest part of them it was fatal.

When I first mentioned my opinion of this disorder to the midwife of a person whom I visited, she said, the woman who drew her breasts was a few days before, on such a suspicion, examined, at the request of a lady of distinction, before she would make use of her; and was declared free from any such distemper; by which means that lady was unhappily deceived, and was one of the last who received the infection. I ordered the woman to be sent to me, and observed a small ulcer at the root of her tongue, and a large recent cicatrice on the inward part of the upper lip. She obstinately denied that she ever had any sore there, but was so much terrified, when I told her she would certainly rot away, that she begged I would not suffer her to perish, if I suspected so much danger. While she was in a salivation, she owned to me and Mr Osborne, an eminent surgeon in this place, that she had an ulcer where the cicatrice remained, which she cured by two or three doses of physic, and a gargle made of woodbine, and some other ingredients, and said she concealed it, because she imputed it only to cold, and was afraid, if known, it might for ever destroy her business and character.

This woman, who communicated this infection to so many, had no eruptions on her body; and, by what I could find, the infection never made any progress beyond the mouth. May
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not this be accounted for from the particular way of life which she followed, the humours being discharged by an almost constant salivation, and the parts deterged and cleansed by a gargle of breast-milk?

Some women, whose breasts were drawn by her, had never any marks of infection; but, by what I could find, the few who escaped were such whom she attended after the large ulcer on her lip was healed; for, while that continued, the nipple was received into a bed of corruption. But the case of a lady was very remarkable, whose breasts were drawn twice a-day by her, when she communicated the infection to all others who came in her way. A violent confluent small pox seized this lady immediately afterwards; and she never had any marks of infection.

The activity of this poison was so great, that I immediately directed a mercurial salivation to even such as were but lately and lightly affected, and ordered it to be brought on by repeated unctions, in a small quantity, with a few grains of calomel internally, and continued the salivation five or six weeks.

I have often observed, that, where the salivation easily rises on the use of the small quantity of mercury, the cure is uncertain, and the symptoms often return: And that the same inconveniencies often attend a large salivation accompanied with a great inflammation, from which many other dangerous symptoms also flow.

The first of these is more frequently the case of women, and such whose fluids are naturally

ly dissolved; and whose vessels are relaxed and tender. The other attends persons of a reverse constitution. In the former, mercury passes off too quickly through the larger vessels, without penetrating into the smaller canals, where the seat of the disorder most frequently lies. In the other, the great viscosity of the humours opposes their dissolution, which must be always previous to a free and effectual salivation; and is absolutely necessary to disengage the infection from them: So that it may easily, by a proper determination, be discharged from the body: For, whenever the animal fluids are heated beyond their natural degree, they immediately become viscid; and all violent inflammations being attended with this effect, must therefore counteract the operation of mercury. Previous warm bathing, gentle evacuations, if there is a plenitude in the vessels, and a diluting regimen, are necessary to prevent these evils; and, after such preparation, a less quantity of the medicine will produce the desired effect with more ease and efficacy.

In the lax habit, neither is bathing, nor much diluting necessary, till the salivation appears; but the medicine must be repeated at a proper distance, and in a small quantity, to answer this end; though, in some cases of this nature, it may, with more success, be determined to the surface of the body, and made sudorific.

The venereal disorder returned to some after a regular salivation, but was entirely removed by the following method, which I made

use of to all who had this infection in a violent degree.

I ordered a machine to be made of oak, the cavity of which was four feet long, and sixteen inches deep, that a person might sit in it with his legs extended; a cover moving in a groove was so adapted, that it closed every part round the body. When this machine was to be used, it was filled to about the height of eight inches, with a strong bath of herbs, with *sal. gem.* dissolved in it.——The process was as follows.

I directed them to take in the morning and afternoon, in divided draughts, a quart of the following decoction.

℞. Rad. opt. bardan. sarsaparil. China glycyrrhiz. an. unc. ii. cum aq. decoct. spatio sem. horæ. Adde ligni rasi santal. rubri, sassafras an. unc. ii. Ebulliant iterum parum, dein decoct. lib. viii. exhibe.

In the evening, about an hour after they had taken the decoction, they went into the bath, as hot as they could well bear it; the lower part of their body being naked, the rest, and especially the head, being well guarded from cold. The steam, which had no other passage but where the opening was allowed for the body, and the heat of the bath, in a few minutes, threw them into a very profuse sweat. They seldom remained in the bath above an half hour; when they came out of it, they were well rubbed near a large fire, dry linen was put on, and they went into a warm bed, where they continued sweating two hours: During this time, they drank very plentifully

of

of three parts water, and one of milk warmed ; their linen was again changed ; they sat up for two hours, and eat a light supper of biscuit, with broth or sack-whey. At dinner, I allowed them any of the white meats, with sack and water for drink.

When they had bathed in this manner five or six times, I ordered a strong decoction of guaiacum to be used instead of the former, and a few grains of calomel to be taken an hour before they entered the bath ; and in some cases I have directed two grains of the turbith mineral to be mixed with the calomel ; which, though continued for a considerable time, seldom affected the glands of the mouth, being determined with such force to the surface of the body. And, if the expression may be allowed, mercury thus managed, produces a salivation through the pores of the skin. When the salivary glands are in the least affected, the use of mercurials is to be omitted, till that symptom disappears.

I have sometimes observed, that the mercurial, taken going to rest the night preceeding the use of the bath, is less liable to affect the stomach, than when taken an hour before bathing.

I ordered the bath three times in a week ; in some cases repeated it five or six times successively every-day, and gradually remitted the use of it.

The patients were seldom faint with sweating, a large supply of milk and water, thin gruel, and sometimes sack-whey, preventing this inconvenience. During the whole course, they

had a very good appetite, and more than usual cheerfulness of spirits. I have known some in desperate venereal disorders, reduced by former methods, acquire a good complexion and strength in this course; and, though I have used it in many advanced and dangerous cases, I never met with one unhappy accident attending it, or with any instance where it failed.

I have of late given *mercurius precipitatus per se* in some cases, instead of the former preparations, as being less liable to affect the salivary glands than any other preparation of mercury. I have frequently directed a grain and a half of it twenty nights following, in a common unguarded way, without observing that it affected the glands, or produced any remarkable sensible evacuation: But lately the same quantity, taken seven nights successively, raised an unexpected and violent salivation in a patient of mine. This, however, has given me a greater opinion of the efficacy of this medicine.

XXII. *A remarkable Hydrocephalum; by Mr JAMES MOWAT, Surgeon at Langholm.*

A Child of a year old is rather less and smaller than she was a month after she was born, having so little flesh on her bones, that she is almost a very skeleton. She has no colliquative stools to keep her from growing, but is only dull and lethargic. She has two teeth in the under-jaw, and two appearing in the upper. When she was a month old, her head began

began to swell, and continues yet daily to increase; it is now twenty-seven inches and a half in circumference; from the point of the nose (which is very much depressed in the middle) to the nap of her neck, twenty-one inches and a quarter; from the one ear to the other, I think it is about eighteen inches. The face above the eyes is exactly six inches broad; the cheeks and mouth are very small for want of flesh; the eyes are large, and she can hide them totally within the orbit, either above or below. All the bones of the face and *ossa temporum* seem to be very large, firm, and broad, without any suture till near the open of the head; but the rest of the head is like a soft quagmire, covered all over with a large, dry, yellow scab. The veins upon the face, and where the skin is free of the scab, are very large, full, and blue; so that they are visible in their minutest ramifications. The child takes no food but the mother's milk: She keeps the head of the child always lying in a hollow between two pillows.

XXIII. *A Hydrocephalum with remarkable Symptoms; by Mr JOHN PAISLEY, Surgeon in Glasgow.*

A Boy betwixt six and seven years of age, of a sound constitution to appearance, and who had been very healthy from his infancy, was suddenly seized one morning with a pain in the left side of his head, attended with an unusual drowsiness and lassitude; which increa-

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fed in the afternoon; his pulse not very quick, short cough, loathing all kinds of meat and drink, an inclination to vomit, with a flushing in his face at times, other whiles pale and ill-coloured, and pains in his belly; the gums of his four back jaw-teeth much swelled, and his mouth hot.

It being suspected that worms were the chief cause of his disorder, some worm-powders were given him, clysters injected, and his belly rubbed with the *unguentum vermifugum*.

The first three days he continued much the same way, being always more brisk and lively in the forenoon, but very dull and heavy in the afternoon. On the fourth day he complained more of the pain in his head, and, being a little more feverish, he was let blood at the jugular, to betwixt four and five ounces, had a clyster injected at night, and next morning got a vomit of ipecacuanna, which operated very well: At the second puke, he brought up a pretty large live worm, five or six inches long, of the teres kind; by this he seemed to be somewhat easier, and more lively all that day.

On the sixth, he got some more doses of worm-powders, though it was with great difficulty he could swallow them, having an aversion to either meat or drink, as well as to the powders; a vermifuge plaister was applied to his belly, and a clyster injected.

On the seventh, his pulse was rather slower than the natural; and though, as was observed, he used to be more quick and lively in the forenoon, yet this morning he was so drowsy and dull, that it was with great difficulty he was prevailed

prevailed on to take a purgative potion: It was much stronger than any he had ever taken, yet had no operation; wherefore he got a strong purgative clyster, by which he had only one stool, being a discharge of some hardened feces covered over with a kind of mucus or slimy stuff, and with them a large worm like the former, nine inches long, came away.

On the eighth he was so dull and sleepy, that he could be diverted no manner of way, nor prevailed upon to take any aliment, and far less medicine: The purgative clysters, continued to be injected, never gave him above one stool. In the afternoon, he seemed lethargic, his pulse slow and unequal: A clyster made of the tops of wormwood and the lesser centaury boiled in claret, was injected, and a bitter infusion in wine was ordered to be taken by the mouth; these raised his pulse a little, and he began to take some panado, or other soft aliment, which he did in a very quick manner, staring very broad, and without speaking a word, but could not be prevailed upon to taste any kind of drink; and, so soon as he had done, immediately fell asleep again.

On the ninth, he was much as the day before, only more comatose: A blistering plaister was applied to his neck, which rose very well, and discharged a good quantity of serum, without in the least rousing him, or making him more sensible, only his pulse was a little quickened.

On the tenth, he was much as the day before, only his face was very florid and red, and somewhat swelled; and he frequently put his hand

to

to the left side of his head, breathing a little quicker than usual, with a wheezing and some difficulty; his pulse slow and languid: Upon which his head was shaved, and the part where he complained the pain was at first, and to which he often put his hand when he could not speak, though there was no appearance externally of any swelling, was scarified and cupped, and three or four ounces of blood drawn away: This eased him as to his breathing, and removed the redness and swelling of his face, but produced no other visible effect; suppedalia were applied at night.

On the eleventh, his pulse was exceeding slow and intermitting, his coma so much increased with startings, that he could take no kind of aliment, and appeared to have no manner of sense; and thus he continued perfectly insensible till next forenoon, when he died.

All along he had such a heaviness in his eyelids, that he could not lift them up with ease, and they appeared swelled.

Upon opening his head, so soon as the cranium was removed, I observed a protuberance or tumor about the bigness of a large hasel-nut, on the *dura mater*, under the parietal bone of the left side, about an inch and a quarter from the sagittal future, and about two inches from the lambdoidal, which was the place the child had pointed his pain was: This tumor had made no apparent pitting in the bone; it felt soft, and, upon opening it, there issued out a little bloody serum, and in the bottom of it I discovered a great number of little white bodies like the small worms in blown meat, or the substances

substances squeezed from persons noses; they had no appearance of life, and were contained in a duplicature of the *dura mater*; the inferior side of which at this part adhered so firmly to the *pia mater*, that it was impossible to separate them without lacerating these tender parts. There were several other smaller ones along the left side of the *sinus longitudinalis superior*, besides the common luxuriant risings of the brain that are always observed in this part; these contained the same kind of bloody serum and white bodies as the large tumor: Whether they were real worms, or an obstruction in the glands of the *dura mater*, following some inflammation in these parts, I could not determine, but rather incline to the last; since, where-ever they were, there was such a strong adhesion of the *dura* and *pia mater*, that they could not be separated without lacerating the tender parts below. All the veins in the head were turgid with blood, as if they had been finely injected, though there was little or none in the other cavities when opened.

When, in taking out the brain, I had cut the optic nerves, I observed a preternatural thickness of the *pia mater*, and a large distension of it as with water; upon cutting it open, about half an English pint of a yellow-coloured water issued out. In dissecting the brain, I found the ventricles had been much distended and enlarged by the water; the *plexus choroides* were hard and schirrous, with a great number of small hydatides (as I supposed) lying along them in rows, whose coats were

were exceeding tender, and burst upon the least touch; they exactly resembled the lymphatics delineated in the fifth table of Dr Ridley's Anatomy of the Brain.

I observed little that was remarkable in any of the other viscera, they being all perfectly sound, except the intestines, which were empty, and in several places inflamed, with apparent signs of a beginning mortification: There were some few of the teres worms in them that appeared dead, most of the small intestines being transparent, and in two places one part was drawn up considerably into the other, like the finger of a glove; as if the peristaltic motion of the inferior part had been inverted, while that in the superior part continued, and so the one thrust into the other: It took a considerable force to draw the one out of the other, the intestine being much contracted in this part, though there was no sign of any inflammation in these parts.

Since this case was under my care, I have seen several children who complained of a pain in one particular part of their head, having a great drowsiness and heaviness in their eyelids, a pulse much lower than the natural, no drought, a great aversion to food or drink, an inclination to vomit, and other symptoms, as this boy had, which made me judge them to labour under the same disease; and the dissection of two, who were all I was allowed to open, shewed my opinion to be just, the parts being found affected in much the manner above described, only that I could observe in neither of them any thing like the little tumors mentioned in

in the former case; and in the latter two, all the vessels of the *plexus choroides* were hard and obstructed, and the *pia mater* at the bottom of the brain, immediately under the optic nerves, was so considerably thickened, as to appear almost like the *dura mater*.

XXIV. *An uncommon Angina; by ALEX. MONRO, Professor of Anatomy in the University of Edinburgh, and F. R. S.*

A Man, thirty four years of age, subject all his life to plethoric indispositions, especially in the spring, and to catarrhs when the least exposed to cold; who had not used any fermented drink, or heating food for several years, on account of a slight hæmoptoe that had more than once seized him, and had retrenched some of his ordinary diet for near two months, to prevent his vernal plethora; having sat several hours in a chamber without a fire, and with an open window, while the weather was very cold and the air foggy about the middle of January 1732, complained, as soon as he rose from his seat, of being stiff with cold and very weary, with pains in moving all his muscles, and perpetual yawning: To free himself of the uneasy coldness, he immediately went home, sat near a fire and drank tea, but could not remove the shiverings and weariness. Upon going into bed he was seized with the appearance of an aguish paroxysm, which had made a regular course before morning.

Next

Next day he was a little feverish, and still complained of pains through all his body, and therefore took his usual cure in slight maladies of that kind, *viz.* a laxative ptizan in which tamarinds and senna had been boiled; which operated gently, and gave him considerable relief.

The day following, he was almost free of all his former complaints, but his right amygdala was swelled and painful, for which the ptizan was repeated.

In the morning of the fourth day, the amygdala was less and freer of pain, and, except a little bitterness in the mouth, he had scarce any other uneasiness. But, in the afternoon, the left amygdala became painful on the least attempt to swallow, his pulse turned quick, but not very strong: He was let blood at a large orifice of a big jugular vein, to the quantity of sixteen ounces; after which his pulse became very quick, strong, and full: On this appearance of plethora, eight ounces more were taken away, and the patient then inclined to faint; but, in a quarter of an hour after, his pulse was very near as strong and quick as ever. The pain of the left amygdala was so far from being diminished, that it continued increasing, and all the teeth in the same side of the lower jaw, that were otherwise very sound, were also violently and constantly affected, but the pains were most exquisite when he swallowed. In the mean time, he could open his mouth as wide as ever, and nothing preternatural could be observed in his fauces, except a small swelling of the right amygdala, and an erysipelatus

erysipelatous redness of the uvula, *velum pendulum*, and left amygdala, without any tumor. After he went to bed, the pains were so rack-
ing, especially when he swallowed his spittle, (which he could not possibly prevent doing frequently, whatever care he took to the contrary), that, though he is a man who is tolerably patient under pain, he made the whole bed shake with his tremblings, and large drops of sweat appeared on his skin at each attempt. About midnight, he could lie no longer; but, putting on his cloaths, he received the fumes of warm water into his mouth; and the pain remitting somewhat, while he endeavoured to hinder the occasions of its violent increase, by leaning his head forward, opening his mouth, and so allowing the saliva to run out, he passed the rest of the night in a drowsy nodding way.

Next morning, his deglutition was performed with great difficulty and pain; and he was so hoarse, he scarce could be understood when he spoke, but had no difficulty of respiration; and the fulness and strength of his pulse were less, but its quickness remained. His throat had the same appearance as the night before. The purgative ptizan was repeated. The fumes of warm water were often made use of, and an emollient cataplasm was applied externally on the pained parts.

In the evening, the pain was easier, and the pulse rather better, but the hoarseness continued. A blistering plaister was applied on his neck and back, and emulsion was prepared for his drink.

The plaister did its office well, and the patient, notwithstanding his pain in swallowing, drank two bottles of emulsion in the night, which prevented any strangury till near noon of the following day, when, the chamber being overheated, and the patient thereby put into a sweat, the strangury came on violently; of which he was relieved, after some hours, by carrying away the fire, and injecting an emollient clyster with turpentine. The emollient poultice was in the mean time renewed, and the fumes of water were frequently applied, and a mild resolvent gargarism assisted the evacuation of mucus, which began now to be discerned in more than ordinary quantity in the fauces. In the evening, the patient seemed by his pulse to be pretty free of fever, and all his troublesome symptoms were considerably removed.

The blistered parts discharging plentifully the two succeeding days, he seemed to mend apace; but still continued the poultice, fumes, and cataplasm.

On the third, which was the ninth of his disease, finding in the morning the pain increased, he took the laxative ptizan, and plied the topical medicines; but through the day the symptoms gradually increased, till, in the evening, his pain in the left amygdala and teeth was rather more violent than ever; the hoarseness was worse than formerly, his pulse was low and quick, he was drowsy, and passed little urine. No swelling could be seen on the left pained side, and he breathed freely. He drank largely of warm posset made with Rhenish wine,

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wine, and chewed long pepper with the teeth of the affected side. His urine soon came in great plenty; he discharged large quantities of mucus at the mouth, and in two or three hours was much freer of pain; his pulse turned slower, and the dozing went off. The gargarism was renewed, with the addition of some *aq. theriac.* and *sp. nitr. d.*; and *theriac. andromach.* was added to the cataplasma.

By the use of these medicines, he was greatly recovered in the two following days, and nothing remaining the third, except a little bitter taste, a trifling swelling in the right amygdala, and a weakness in the left side of the fauces: He went abroad to his usual labour of dissecting in the forenoon, and prelecting in the afternoon, which he continued to do for a week, using a spare diet and applying the topics; in which time he got quite free of all uneasiness, except a dull pain of the left side of the fauces, especially in yawning; and he imagined all the food he swallowed passed only by that side, where sometimes a little of it stopt, and was with some trouble pressed back again into the mouth.

To remove this weakness, the patient one evening gargled his throat with claret wine and a little *aq. theriac.* mixed. That night, he was sensible of a straitening in his breath; and in the morning when he awaked, he was quite hoarse, breathed with more difficulty, and, in coughing, his head and eye-balls felt as if strongly girded, his face became very red, and he began to hiss in breathing. All these bad appearances went greatly off, after breath-

ing in the fumes of warm water, and drinking warm tea. In a few hours after, he swallowed pills composed of *mercur. d.* and refined of jallap, which operated mildly, and lessened all the causes of complaint considerably, and a second such dose, taken two days after, removed all of them entirely, except a little weakness of the left side of the fauces, and an obtuse pain there in yawning, which remained several weeks after.

In the history of this disease, I have given a strong hint who the patient was, and suspect the common frailty of enlarging on one's own misfortunes may have discovered sufficiently, that it is my own case I have related.

What was the particular feat of this anomalous angina? Will the uses assigned to the digastric muscles in Art. XI. of Vol. I. be of use to discover it?

XXV. *An Asthma, with uncommon Symptoms; by the same.*

MR James Ure, whose business as a writer, or solicitor at law, engaged him frequently in company and good fellowship, was sometimes troubled with a palpitation at his heart, a trembling in his hand, and faintness; being always subject to a cough, and having generally very little appetite for food, with two or three loose stools a-day. In October 1730, the fifty-fifth year of his age, having caught cold, a cough seized him, with which he brought up a small quantity of very thick slime, and complained

plained of a difficulty of breathing; he lost all appetite for food, and his belly became bound; his urine was in small quantity, and he could not feel his own pulse.

He allowed these symptoms to continue eight days without asking any advice; but, finding them continue, and rather increase, he consulted Dr. William Porterfield and me, October 20. Besides the above-mentioned symptoms, we observed his difficulty of breathing increase so much upon lying down, that he was obliged to sleep in a sitting posture. His feet and legs were very oedematous. We could feel no pulse in the arteries of his wrist, neck, temples, or ham, but only a sort of trembling under our fingers, which we then attributed to some irregularity in the distribution of his arteries, notwithstanding his affirming, that, when in health, he had a pretty strong pulse in the very parts we felt. The veins of his arm and neck were very large, and stretched with blood. He could take no sort of food, but had a thirst, which was not violent, nor was his tongue dry, or his skin hot. When the cough attacked him, he forced very little puit up; but his breathing became exceeding laborious, his face turned very red or purple, and he complained of a violent head-ach. When he was free of the cough, a little while he walked, spoke, complained of little, and assured us he was scarce weaker than in health. To prevent the bad effects of the cough, twelve ounces of blood were immediately let; he bled freely, and did not turn faint. In the evening, a terebinthinat purging clyster was inject-

ed, which purged him gently. He was desired to take any sort of mild food which his stomach could receive. Rhenish wine and water were given for drink, into which he frequently dropped forty guts of the following mixture. *℞. Elixir pectoral. unc. i. Tinctur. castor. drach. ii Sp. salin. aromat. drach. iii.* and every two hours he swallowed two spoonfuls of the *oxymel pectoral.* of the Edinburgh dispensatory. At bed-time, he took this bolus: *℞. Sperm. cet. scrup. i. Sal. succin. gr. v. Conserv. ros. scrup. i. Syrup. alth. q. s. ut fiat. bol.*

Next morning, there appeared no change in his condition; and he took *p ipecacuan. gr. xxxv. Oxymel scillit. unc. i.* This emetic operated six times, without increasing his headache or difficulty of breathing, and brought a good deal of slime with the water he drank. He seemed a little relieved after this vomit, and continued the use of the medicines prescribed the day before. At night, a blistering plaster was applied to his neck and shoulders; he took his bolus, and had a cooling emulsion for drink.

The blister rose well, but the pain of it made him so unable to move next morning, that we could not give the purgative we had determined to order. His urine was in larger quantity, with a good sediment; notwithstanding which, his legs were rather more swelled, and the other symptoms continued. His bolus was again given in the morning; he still drank water with Rhenish wine; and he was desired to take frequently a spoonful of the following

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following mixture, *R. Aq. hyssop. puleg. a. unc. iii. Cinam. f. v. raphan. compt. a. unc. i. Gum. ammoniac. drach. ii. Sal succin. drach. sem. Oxymel. scillit. unc. ii. Syrup. alth. unc. i. M.*

He continued much in the same way all the 22d and 23d, only his legs and thighs swelled more; and he began to complain that his cloaths were too tight for his belly.

In the morning of the 24th, he took this purgative bolus, *R. Pulv. rhei elect. scrup. i. Aquil. alb. pulv. jallap a. gr. v. Syrup. de rhamn. q. s. ut. ft. bol. capt. e syrup. violar.* This purged him mildly, and the quantity of his urine increased considerably, and his belly and legs became less swelled, which relieved him somewhat of the orthopnea.

Next day, he began to use a medicated wine: *R. Rad. irid. Florent. scill. acor. ver. helen. raphan. rust. a. unc. sem. Gentian. drach. ii. Rub. tinctor. unc. sem. Cortic. sambuc. ebul. a. unc. sem. Herb. marub. alb. m. ii. Absinth. rom. centaur. min. a. m. Sem. bacc. juniper. unc. i. sem. Sal. tartar. drach. iii. Incis. & contus. f. a. M. ut ft. malia infundend. in vin. Lisbon lib. vi. Colatur. capt. unc. ii. mane & hora 5ta vespertina.* All this day and the following, his urine passed plentifully, with a large sediment; the swellings diminished, and his cough and breathing were easier. In the evening of the 26th, while I sat by him, he surprised me with putting his wrist into my hand to feel his pulse, which was full, strong, slow, and equal; but, two hours after, I could again

again feel no more than the very weak trembling I formerly mentioned.

The 27th the purgative bolus was repeated. On the 28th, his pulse returned, and continued afterwards firm and strong, till he recovered his former health, which he did in a little time, by the use of the medicated wine, pectoral mixture, and the purgative sometimes repeated.

About a month after, having got the cold again, his symptoms began to return with the same state of his pulse; but, upon taking the former emetic, and the purgative, they went off; and, to prevent a second relapse, he got strengthening chalybeat medicines, with the stimulating gums, for some time.

In the beginning of January 1732, he was again attacked with the asthma, and want of pulse; which were removed with two doses of the purgative. After a cough, which continued some days in the beginning of February, his pulse could not be felt during twenty-four hours, but it returned after the operation of a purgative bolus.

Soon after my patient retired to the country, where he has continued in very good health; and being lately in town, revised this account of his case, and allowed me to communicate it to you.

XXVI. *A large Steatom passing with the Oesophagus from the Thorax into the Abdomen ;*
by Mr JAMES JAMIESON, Surgeon at Kelfo.

A Carpenter in this place, thirty-six years of age, of a thin make of body, asked my advice about the middle of February 1732 ; his complaints were, pains at the heart, in the pit of the stomach, both sides of the thorax, and betwixt his shoulders ; a great difficulty in swallowing, and frequent inclination to vomit. He told me it was about six years since, upon a sudden grief, he was first seized with the pain, and frequent palpitations at his heart ; and that the other symptoms came on gradually afterwards, but never kept him from his ordinary work till now.

At my first visit, I took twelve ounces of blood from his arm ; the blood was fizy like that in a rheumatism. Next day, he took *pulv. Ipecacuan. drach. sem. Tartar. emet. gr. ii.* which purged him four or five times, but did not occasion the least nausea. I gave him also doses of the *pillul. gummos.* twice a day, washing them down with *decoct. amar.* in which a small quantity of *sal. absinth.* was dissolved ; caused the pained parts to be fomented with a strong decoction of the aromatic plants, and applied a large plaister of *theriac andramach. ol. mac. per express.* and the powder of the carminative seeds. The bleeding gave him some remission of pain, and was repeated twice in the
two

two following weeks, with some short relief each time.

Finding, however, his complaints rather increase, I obliged him to call a physician, which his poverty had made him obstinately refuse to do hitherto: And, while Dr Abernethy was sent for, he shewed me a hard painful tumor, immediately below the *cartilago xiphoides*, which, he said he had only discovered the preceding night.

The doctor suspecting, from the account the patient gave of his disease, and from the appearance of this tumor, that an abscess was forming in the stomach, ordered him a milk diet, and linseed tea, or a decoction of *hedera terrestris*. or milk and water for ordinary drink, and caused an emollient cataplasim to be applied to the tumor.

He continued to turn gradually worse, till about the middle of March, when he was attacked with a vomiting, which none of the different stomachics nor opiats given him could restrain, but were thrown back again as soon as swallowed. His thirst became excessive and perpetual; but all his drink, though taken in very small quantities at a time, was vomited as quickly as his medicines, without increasing his pains; and a singultus soon came on, which continued till his death.

He passed very little feces by the anus, and these were generally procured by gentle clysters.

His urine was for the most part limpid; when there was any sediment, it was white.

During

During the whole course of this disease, the patient's pulse was never quick or strong, but low and languid, and sometimes intermitted. His pains were always most severe in the night, and at last deprived him almost entirely of sleep; and, for a whole month, he did not lie down, but sat in the bed, or on a chair, bended forwards with his head on a pillow. In this melancholy condition, he languished till the first of April when he died.

On laying open the thorax and abdomen, we observed a prodigious large steatomatous body, which filled a great part of the thorax, being grown to the pleura in both sides, to the larger share of the mediastinum and pericardium; and then accompanying the oesophagus, they passed together through the diaphragm, having enlarged the passage in this muscle considerably, and straitning the oesophagus. After entering the abdomen, it was stretched along the dorsum of the stomach to the pylorus, which it also compressed greatly: Both the orifices of the stomach being so much contracted, that I could scarce push my finger through either. I was about to cut out this whole tumor, to measure and weigh it, but was opposed by the relations; and therefore behoved to content myself with examining its substance as it lay in the body. It was so hard, my knife could scarce cut it, but, after a transverse incision, several sinuses, formed in the firm white substance, discovered themselves; some of them contained a matter like a meliceris, in others, it was a-kin to that of the atheroma, and in a third sort it was purulent and foetid.

The

The fat of the omentum was all wasted, but all the other viscera were found enough.

XXVII. *Of the Service of a warm Bath in a bilious Colic*; by ROBERT PORTER, M. D. Member of the College of Physicians, London.

IT is not my design to describe the several kinds of colics, their contradistinction from each other, and their different methods of cure; my purpose being to recommend one particular method of managing the bilious colic, and those severe ones, whose cure depends on procuring an entire thorough discharge of that acrid matter within the intestines, that causes the disease (though not properly bilious, because unattended with such vomiting) which seems but little attended to; yet will appear from reason and experience to be so highly useful, and so absolutely necessary, that this distemper ought never to be treated without such additional assistance.

The description of this disease by Sydenham * is so just, that in this point nothing can be added to him. It will also readily be confessed, that the grand indication of cure is to obtain an open passage through the intestines for a perfect discharge of that acrid irritating matter contained within them. As this therefore is the true cause of the disorder, to the grand point of its evacuation should the whole method be directed.

But

* S enh. § 4. cap. 7.

But such is the alteration produced in the cavity of the bowel, by the constant sharp stimulus of this acrid matter, that it is not only contracted into an unusual narrowness; but, if the observation of authors of unquestioned veracity may be credited, the coats of the affected intestine have been found, upon dissection, so closely joined, and so entirely precluding any downward passage, as if they had been strongly girt round with a ligature.

But the circumstances of the ilium bowel, produced by this sharp irritation in these colics, will be perfectly understood, and cannot possibly be better explained, than by transcribing that most beautiful passage from Peyerus, where he relates an experiment he made on a living frog, that happily sets this matter in the clearest light; not only visibly explaining the effect of a severe colic on the intestine, but representing, at the same time, the manner in which its frequent direful consequence, the iliac passion, is produced.

“Torminum genesin, atque intromissionem
 “conceptionem, anni superioris æstate, rana
 “jucundo admodum et utili experimento oculis
 “nostris exhibuerunt. Etenim intestina, vi-
 “vente amphibio, lacerata in diversis locis, per-
 “tinacissimè mox se constringebant, contentis
 “violenter sursum deorsumque, quâ data porta,
 “protrusis, atque hinc inde in cumulos quasi
 “congestis: Unde quædam intestini portiones
 “valde impletæ ac turgidæ, quædam prorsus
 “inanes et arctissimè clausæ persistenterunt; donec,
 VOL. III. E e “soluta

* Peyer. de Gland. intestin. cap. 9. p. 81.

“ soluta fibrarum stric̄tura, meatus liberior red-
 “ deretur: Intestinis autem hinc inde se con-
 “ stringentibus, et sive chylum, sive fœces sur-
 “ sum deorsumve projicientibus, factum, ut
 “ illæ alicubi in molem adgestæ parietes inte-
 “ stini plus debito ampliarent: Quod in sacci
 “ morem dilatatum intra se recepit constrictam
 “ inferioris intestini portionem, eamque sinu
 “ suo absconditam aliquamdiu retinuit: Donec,
 “ fibris se denuo exporrigentibus, intestinum,
 “ è latibulo alterius, in apicem pristinamque
 “ sedem rediret.”

If such then is the bowel's contraction in the severity of this disease, one grand important point to facilitate this evacuation, on which the cure must turn, seems to be the removal of that stric̄ture; and, by relaxing the tightness of the intestine, to procure a freer and wider passage for the discharge. For, unless this be jointly endeavoured, in conjunction with the other method of cure, I apprehend we do not assist the patient with all that our art might contribute to his relief. Nor do I know any thing so effectual to this desirable end as the warm bath, which is daily found so highly advantageous in somewhat a similar case, I mean the discharge of gravel from the kidneys. I am not so foolishly prejudiced to imagine the bath alone would prove effectual, or ought singly to be confided in for the cure of these colics: I hope I shall not be so understood; the whole I would endeavour is to propose and prove it a very efficacious assistant, joined with the other known and usual methods, and what will greatly forward the happy effect of them.

But

But, since I have mentioned a nephritic disorder, as somewhat a parallel case, I beg leave to run over the several indications of cure in that disease; from whence the near similitude of managing both will readily appear; and the advantage of the warm bath in one be no small encouragement to its use, and evidence of its service, in the other; because the manner in which it is useful is in each the same.

It will be readily confessed, the grand point here is the evacuation of that fabulous matter lodged in the pelvis of the kidneys, or stuffing up the ureters. The methods to effect this are, immediate bleeding, to remove the tension and inflammation of them; by widening, as much as possibly can be, the capacity of the ureters, for the passage of these gritty concretions; for which purpose the injection of emollient clysters has a double advantage; both as they convey an internal fomentation to those slender tubes, by this warmth relaxing their contraction, and, by unloading the lower bowels of any accumulated fœces, they remove their lateral pressure against the ureters. To open yet more this passage, the warm bath proves of most important service: For, by this warmth and humidity, the abdominal muscles, peritonæum, and intestines, are greatly relieved from their tenseness; and the diminution of their former pressure allows a readier discharge of the gravel. Hence also the bladder is considerably relaxed; consequently the oblique insinuation of the ureters, thro' its several membranes, is less liable to obstruct the evacuation of this sandy matter into its cavity.

By moderate diuretics, by the use of oleaginous, emollient, and deterfive medicines, this discharge is greatly assisted, and the passages themselves at the same time properly lubricated, to facilitate the expulsion, while the severity of pain is suspended by anodynes, that produce also a kind of paralytic resolution of the spasmodic contraction of the ureters on the gravel (by which convulsive effort to expel, they entirely preclude its descent); and therefore contribute not a little even to open the passage.

These appear the most considerable methods for the relief of this disorder, which is but imperfectly managed, without the united assistance of all; and which, used together, seem the utmost art can furnish.

Let us now examine the methods of relieving these colics. By instant bleeding, the tension of the bowels is in some measure eased, a timely revulsion is made from the blood's impulse on the affected part, an inflammation, with its consequences, is prevented: By giving immediately brisk cathartics, we propose to urge forcibly forward the obstructed, acrid, morbid matter; and, by the subsequent use of more lenient and less stimulating medicines, to assist the operation of the former with less violence; and, by the joint assistance of anodynes, not only to procure a suspension of pain, but to diminish the convulsive contraction of the intestine. So far the curative intentions in either disease seem to quadrate with each other. The injections indeed of clysters here are of little significancy; for they cannot pass beyond the valve of Tulpinus, placed at the ilium's entrance into the cæcum; con-

se-

frequently their warmth cannot relax the affected intestine, nor their purgative quality dislodge the obstructed matter. Still the patient and physician continue alike disappointed: the excess of pain remains, and the body, obstinately co-flive, eludes the united effort of this method. The repetition of the more violent purgatives, by the strength of their acrid stimulus, provokes a firmer tightness and contraction; and by this means they partly frustrate their own operation; yet the milder have already proved too weak to affect any thing. By enlarging the anodynes, his pain is but momentarily mitigated; and even these, by the costiveness they occasion, are liable in some degree to prevent a discharge; yet the patient can never be safe, nor can the disease ever be cured, without procuring a thorough evacuation.

But what will be the consequence of this obdurate obstruction? Either certain death, or the contents of the smaller intestines, the medicines and the liquids the patient takes, if not returned by vomit, are together stopped at the contracted part, and, with the rarified air contained here likewise, dilate greatly the upper portion of the bowel to a very wide amplitude, while that below the contraction is empty, close, and undistended; till, by violent straining in the torture of this agony, the contracted part of the intestine is forced upward and inward within this widened part, too greatly stretched not to admit a ready intromission, one being so preternaturally straitned, and the other immediately contiguous so vastly widened

beyond its natural dimension. This is the true, the formidable iliac passion, in which the vessels surrounding the coats of the intestine are doubled, the circulation through them instantly prevented, and a swift mortification immediately follows. All which might certainly have been prevented, if the single point of the intestinal stricture could have been removed, to make way for the evacuation; nor could the strongest cathartics forcibly break through it. Should we not then attend to this point with great diligence, and apply to obtain it, a method so certainly serviceable in the nephritic case, by enlarging the ureters? Sydenham * indeed, in his discourse on the iliac passion, which he supposes produced by the sharp irritation of acrid matter, seems partly to propose relaxing the contracted bowel, by this method of applying an animal warmth to the pained part of the abdomen, laying a large puppy on it; though I confess his principal view appears to have been by this heat to strengthen the weakened intestine, agreeable to what he lays down as the second indication of cure; but he hints at no such indication in his chapter on the bilious colic: Yet here only is it likely to be most advantageous, before the case has proceeded so far as to terminate in this dreadful symptom.

Shall we imitate this truly great man, and endeavour to improve on his method, by fomenting with emollient decoctions the pained part, and thus endeavour to direct and determine their effect to the very point where the severity of
pain,

* Sect. 1. cap. 4. p. 41.

pain is complained of? But this will prove an insufficient, a defective, and a partial method, while an immersion in a warm bath, prepared of the same kind of decoction, is an universal focus to the lower trunk of the body: Besides that, such a quantity of relaxing moisture cannot be imbibed by the bibulous vessels, from the expressed flannels, as must be absorbed from the surrounding fluid itself; where, used as a bath, the application to the part is the same, the warmth is equal, and the extent of its relaxing influence perfect and general on all the abdominal region. It may also be worth inquiry, whether the pressure of the water has not some superior influence to urge a larger proportion of fluid into those absorbent vessels.

We see then how nearly allied these two distinct and widely different diseases are in their respective curative indications; the grand article of relaxing, opening, and procuring a free passage being of equal importance in both. I dare not contend indeed, that the decoction of emollient plants actually carries any superior virtue in it beyond simple warm water; for the gentle heat and humidity of the latter may prove as sufficient for the purpose: Yet, as the worst that can be said of this artful disguise is, that it is a needless pomp, the *pia fraus* deserves to be continued; for the patient would be too apt to entertain a low, a thankless opinion of his physician's merit or depth, that effected his cure by so simple, so naked a remedy.

Upon the whole, I would not be apprehended to advance the notion, that no bilious colic can possibly be cured without warm bathing:

The

The contrary is every day experienced ; for, in the milder kind, where the intestinal stricture is far less straitened, the obstruction more readily yields to the force of cathartics : Yet, even here, in proportion to the abatement of this tightness, the more speedily will it assist in resolving it, the more conducive will it be to quicken the operation of internal medicines, to hasten the patient's relief, and prevent any subsequent danger ; for which reasons I should not, even in such case, and at the beginning of the disease, omit it : And I am persuaded, in the severer kind, necessity indispensably commands us to use some method for relaxing the bowel, that an evacuation may be timely obtained ; and cannot but believe many an iliac passion might have been happily prevented by an early conjunction of the warm bath with the other manner of cure.

But it is justly expected from every medical writer, that he should confirm and establish, from experience, and the authority of repeated trials, the real efficacy of that method he recommends ; for, unless the sanction of fact is on his side, all the flourish of laboured reasoning, and pomp of probable argument, is but ridiculously vain ; a barren superfluity of words, a *vox & prateria nihil*. But this, though a necessary, is yet a painful task to every modest author ; for even the simple narration of successful truth is so frequently construed a vain glorious boasting, and carries such an air of ostentation with it, that the censorious world too often, sometimes too truly, concludes the history was not so much added to confirm the preceding

ceeding argument, as the argument designed to introduce the subsequent self-applauding story.

I shall however give some few instances, wherein I have experienced the success of this collateral aid of the warm bath: The first I produce is the case where I first used it for this purpose; and the happy instantaneous relief was, as far as a matter of this nature can be proved, evidently and entirely owing to it.

July 13. 1731.

A gentleman of Spittlefields, about thirty, of a moderate habit of body, by drinking adulterated bad wine in a journey, was immediately seized with a violent pain in the smaller bowels; which increased daily, attended with an entire costiveness: On his return, he immediately applied to his apothecary, who prudently attempted to remove the obstruction by various pertinent methods; as bleeding, and the stimulus of rougher cathartics: Which proving ineffectual, he tried the laxative method of more lenient medicines, a solution of manna in purging waters, attended with oily draughts; not omitting the proper use of opiates, to relieve the convulsive spasm of the intestine; he had also injected several clysters of the softer and the strongly irritating kind.—In vain. I found him in the utmost acute pain, as if a cord had been forcibly strained round the abdomen, attended with frequent vomiting of yellow, viscid, bilious matter; and attempted once more the effect of a smart cathartic, (*Extract. rudii drachm. sem. Calomel gr. xv.*) followed with a decoct. of *Fol. fenn. and rad. rhei*, with the addition of
Elix,

Elix. salutis and *syr. ros. solutiv.* of which he took two spoonfuls every hour. I waited on him in the evening, but he had received no manner of benefit from this method, his torture continuing without any abatement, with the same inexpressible agony. I resolved therefore to try what might be the advantage of a warm bath, by relaxing the intestine, and opening the passage: One was instantly prepared from a decoction of the most emollient plants; and such was the happy consequence, that, even while he was in it, he had a loose stool, soon followed by five more, though he had but one for ten days before. This copious discharge terminated his disorder; the remaining tenderness of his bowels being soon relieved by an opiate, and a solution of *sperma ceti*.

August 28. 1731.

I visited——Eaton, a child not quite five years old, who was seized the 22d of the same month with a violent colicky pain immediately after eating two large raw codlings: Several clysters had been ineffectually thrown up, and several cathartics as fruitlessly taken internally; for the child had had as yet no stool: I found the abdomen greatly swelled and hard, with excessive pain; the pulse was frequent and strong; the respiration quick, laborious, and indeed struggling. Bleeding was immediately performed; and the blood drawn away was covered on its surface with a leathery substance, equally thick and hard with that of any adult, robust, pleuritic patient I had ever attended. He took instantly *pil. coch. min. gr. viii. calomel.*

lomel. gr. iii. in the form of pills; and within an hour began the use of the mixture following,
R. Fol. fennæ drach. i. Rad. rhei scrupul. i. Coque in aq. fontan. q. s. Colaturæ unc. iii. adde elix. salutis, manna an. unc. sem. M. Sumat. cochl. ii. omni hora donec responderit alvus. A warm bath was prepared from a decoction of emollient plants; he drank also frequently a solution of *manna unc. sem. in unc. iv. of decoct. pectorale.*

In a few hours after twice using the semicupium, and taking a proportional quantity of the other medicines, the obstruction was happily removed, and five copious dejections concluded the disease; the greatest part of the apples returned crude and unaltered, with the stools.

October 3. 1732.

—— Caffeck, a plumber about forty, was seized September 30th with a violent colic, soon aggravated to great intenseness of pain; either alternately fixed and contracted to a point, or strongly surrounding the abdomen like a tightened girth, and had received no evacuation by several cathartics and two clysters administered to him. The keen severity of his pain extorted loud and constant screams from him. His pulse was strong and full; wherefore blood was immediately drawn, and a dose of *pil. coch. min. cum calomel.* prescribed: An hour after which, he entered on the use of the following medicine:

R. Fol. sen. drach. iii. Rad. rhei drach. i. Coque in aq. fontan. q. s. Colaturæ unc. vi. adde elixir salutis unc. i. sem. manna unc. i. Sal.

Glauberi

Glauberi unc. sem. M. Sumat. cochl. iii. post elapsam a pilulis horam, dein cochl. ii. omni hora. He used the semicupium, made of emollient herbs, twice a-day, half an hour each time.

Fourth, His agony increased so violently last night, that five men could with difficulty overpower his struggling, and detain him in bed: To quiet this severity, I ventured on *gr. i. sem.* of crude opium; by it his pain was greatly mitigated this morning, but without any stool. He continued this day in the constant repetition of the same opening mixture, and the bath, as before: Whence, the morning of the next day, he had five large stools, and with them an end of his misery and disorder. A solution of *sperma ceti* finished his cure, by removing the remaining soreness of his bowels.

May 3. 1733.

Lord, a plumber also by business, a robust man, about thirty-five, was taken with a strong pain in the smaller intestines, April the 29th, that seemed like a tight bandage to gird the abdomen round. He was the patient of an apothecary, to whom I had frequently and strenuously urged the use and necessity of warm bathing in severe colics: He had therefore from the beginning prudently joined this with the other method of strong cathartics, but without any effect; I found his pulse hard and strong, his pain increased to perfect agony, either determined to a point, and piercing his body thro', or at other times binding forcibly the abdomen round. I attempted his relief in the following manner:

Extra-

Extrahantur e brachio sanguinis unc. xvi. statim. ℞. Extract. rudii scrup. i. Calomel. gr. xv. f. Pilula statim sumend. superbibend. prorsus nihil: Tribus vero elaps. horis, capiat coch. ii. mistura sequentis. ℞. Fol. sen. drach. iii. Rad. rhei drach. i. Sal. Glauberi unc. sem. Coque in aq. fontan. q. s. Colatura unc. vi. adde elixir. salutis unc. i. sem. Mannæ unc. i. M. Capiat cochl. ii. omni hora. Pergat in usu semicupii. This be used half an hour twice or thrice a day.

Fourth, As yet he found no lasting abatement of pain, though the bath constantly mitigated his agony, while he continued in it; nor was any stool obtained, though he had taken the whole of the opening mixture: Wherefore the following more quickening medicine was prescribed: *℞. Extract. rudii, calomel. an scrup. i. Resin. jalap. gr. vi. f. Pilula quamprimum sumend. ℞. Fol. sen. unc. sem. Rad. rhei drach. ii. Sal. Glauberi drach. vi. Coque in aq. fontan. q. s. Colatura unc. ix. adde elixir. salutis unc. ii. sem. Mannæ unc. ii. M. Capiat cochl. ii. post tres, ab assumptis pilulis, horas, & dein omni hora.*

He still kept to warm bathing as before. I visited him again in the evening, but found no benefit yet received, the intestinal obstruction continuing equally obstinate, with equal pain. His pulse remaining still full and hard, *unc. xii.* of blood were again drawn away, which, like the first, was greatly inflamed; he continued still in the constant use of the aperient mixture. The next morning his body was happily opened; yet not before a consumption of one and a

half of the last prescribed medicine : So difficult was it to get an open passage ! Yet the following day our patient complaining of a pleuritic pain, and his pulse continuing very hard, strong, and full, his vein was opened a third time with success.

I have thus honestly related a few cases, wherein, I apprehend, the warm bath did considerable service : Its remarkable advantage in the first instance sufficiently encouraged me to make repeated trials of its efficacy ; and I am convinced it ever greatly assisted and promoted the operation of cathartics, and greatly forwarded the patient's recovery.

I suppose it will scarcely be urged on the contrary, that it as evidently appears from most of these very histories, that the bath was frequently repeated before the work could be performed ; consequently its virtue was of little avail, nor worth the trouble of preparing : But, where the obstruction is great, where the tightness of the intestines is obstinately firm, the strength and repetition of cathartics must be proportional, the frequency and continuance of bathing must be kept to without weariness, that the virtue of this united method may at length prevail. In nephritic cases, the bath, and all the other known methods of cure, must sometimes be long persevered in, before the fabulous matter, lodged in the kidneys, or plugging up the ureters, can be brought away ; because, therefore, it has not an immediate effect here, we shall not rashly pronounce it of no benefit : For, as in the intestines, the relaxing every
stricture,

stricture, and opening every obstruction, will be earlier or later, as is the degree of their strength, duration, and tightness.

On the other hand, I am far from attributing these recoveries singly to the bath, exclusive of the help from internal cathartics. But, when I consider the very nature of this distemper, the manner in which it affects the intestine, the continuance of the obstinate costiveness, the severity of their torture, (ever mitigated by bathing), and the large quantity of cathartics of either kind taken before the passage was made, I cannot help concluding the semicupium was really and highly useful in their relief; greatly forwarding their cure, and preventive of worse consequences that would have arisen from a continued unremoved obstruction. In this single view, of an assistant to the other methods of cure, I endeavour only to propose it.

XXVIII. *The Water of a Dropsy evacuated at the Navel; by Dr FRANCIS PRINGLE, late President of the College of Physicians at Edinburgh.*

IN June 1726 I was desired to visit a woman betwixt forty and fifty years of age, who had laboured under a *hydrops ascites* some years; and in that time had taken a great many medicines by the direction of a physician. The most powerful diuretics and hydragogue purgatives were given by another physician and me in vain, the bulk of her belly which was very great when I saw her first, rather increa-

sing; so that at length, despairing of doing her service by medicines, and she refusing to undergo the paracentesis, we gave over prescribing for her.

Having one day taken a pretty brisk purgative, she told me that the waters ouzed at her navel: This ouzing continued constantly after, but was slow and gentle, giving her no other uneasiness than what was occasioned by wetting her linens. She remained in this state all the winter, without her belly increasing, but her flesh and strength wasted.

In May 1727, having gone to Leith in a coach, in returning home, the orifices at the navel were so dilated that the water gushed out in a stream, as if she had been tapped; and with great difficulty she was carried from the coach to her lodgings, where the waters continued to flow plentifully, while I was sent for, the people about her computing that she had discharged between 12 and 16 Scots pints of water. When I came, she was so faint, her pulse so sunk, and her looks so ghastly, that I found it absolutely necessary to put a stop to any farther discharge of water at that time, by proper compresses and bandage, and then ordered her to be laid in bed with her head low and her feet raised. She passed the night indifferently, but recovered her pulse and spirits. Next morning, a chopin more of water ran out, when the wet dressings were changed for clean ones, and that day she was seized with a fever, attended with violent and threatening symptoms, such as vomiting, hiccough, and asthma. The fever continued in this way
some

some days ; but at length she recovered entirely, and continued that summer and most part of autumn in a perfect state of health. Her belly not being the least swelled, and her flesh, strength, colour, and appetite being recovered.

In October, she was seized all of a sudden with a colic, attended with violent vomiting, and an obstinate constipation, which cut her off in a few days.

Upon opening her body, there was no water nor any remarkable disorder found in the abdomen, except that the uterus was vastly large, being schirrous all over, with its coats greatly thickened. It weighed four pounds and an half.

XXIX. *The Menstrua regularly evacuated at an Ulcer of the Ankle; by Mr JAMES CALDER junior, Surgeon in Glasgow.*

A Healthy, vigorous, labouring country girl, of an ordinary stature, strained her right foot, at fifteen years of age, and again at nineteen, when a sordid ulcer broke out in it: This being healed up in three weeks, she soon after complained of a disorder through all her body. At twenty years of age, her menstrua appeared for the first time, but in very small quantity.

The former disorders still continuing, she was blooded at the *vena saphæna* of that right foot. Soon after which an ulcer was formed in that ankle, which has now continued above five years, a considerable share of the *os calcis* having come out. This ulcer sent out in two

or three days of each month, as large a quantity of blood, as women generally pass in their courses, and this in regular periods, without any bleeding at the intermediate time. Some days before this periodical hæmorrhage, she always complained of great pain in her foot, which became tolerably easy, as soon as the evacuation was past. She continued in this way till, in May 1733, the bones being cast out, and the ulcer beginning to heal up, while her body turned more plump and strong, the menses came the natural way, in much the same quantity, and continuing the same time they used to do at her foot, which bled none. She had a second natural return of the courses in June, but, in July, she passed the natural period, and her foot became more painful; but, the menses returning as in other women, the pain ceased; and she has ever since that time been in good health, with her menses regular in the natural way, without any other evacuation than of a small quantity of pus at the ulcer, which still continues a little open.

XXX. *An Account of Medical Discoveries, Improvements, and Books published in the year 1732, and omitted in the second volume of this Collection.*

Discoveries and Improvements.

Several anatomists, particularly Messrs Hister and Palsyn, having of late disputed, whether Steno's duct, between the nose and mouth,

is

is pervious in the recent subject, as well as in the skeleton; Dr Kulm, professor of medicine at Dantzic, affirms, that he has found and demonstrated it to several to be pervious in a deer, a bear, wild goats, hares, calves, dogs, and in the human subject; and then mentions the manner of tracing it. He says there is a double opening of this duct into the mouth at the papilla, immediately behind the *dentes incisores*, out at which orifices a liquor bubbles, when the palate is pressed with the fingers from behind forwards, by which one is directed to introduce a hog's bristle into these canals; and by pushing the bristle first perpendicularly, and then a little backward, it very easily passes into the nose. Besides, says he, the place of these ducts in the nose may be readily distinguished by its greater declivity, *Tabul. Anat. XI.*

Dr Pozzi, professor at Bologna, endeavours to prove, in the *Commentariolum* tacked to his orations, p. 55. that the nails grow out from the tendons that are spread on the last phalanx of the fingers and toes.

P. 58. He says, That the thymus of a calf, softened by maceration, discovers a lobe from which a milky liquor runs out when it is wounded; and, if air is afterwards blown into it, the whole thymus is distended, and may be dried, when it plainly appears to be composed of cells communicating with each other; upon the sides of which muscular fibres are to be seen. From which structure he concludes, that the thymus in foetuses supplies the use of the lungs, serving as a receptacle for the chyle to be prepared in.

P.

P. 72. Our author relates the phænomena he remarked in dissecting a dog, whose spleen was cut out when he was very young. The liver, which was larger, and more heavy than ordinary, was also become more brittle. The *vena portarum* was enlarged. The gall-bladder was full of bile; the colour, consistence, and taste of which discovered it to be more acrid than it is commonly. From these appearances, he is led to think the use of the spleen to be for separating a liquor like to spittle, which may dilute the bile, and prevent its too great acrimony.

B O O K S.

Differtationes duæ, de viribus medicatis olei animalis in epilepsia, aliisque affectibus convulsivis, 8vo, Londini.

A new English dispensatory, by James Alleyne, M. D. 8vo London.

Thomæ Fieni libri chirurgici duodecim de præcipuis artis chirurgicæ controversiis, editio 2da, 4to Londini.

Profodia chirurgica; or a *memoria technica*, calculated for the use of old practitioners, as well as young students in surgery; being a lexicon wherein all the terms of art are accounted for, their most received sense given, and an exact definition of them from the best Greek authors: Also their pronounciation as to quantity determined by proper marks over each syllable. The second edition, 12mo, London.

Giusti-

AND OBSERVATIONS. 345

Giustificazione di Antonio Benevoli, Cerusico e maestro delle insigne spedale di santa Maria nuova del città di Firenze, dalle replicate accuse del Signior Pietro Paolo in Lucca, 4to, Florent.

Antonii Vallisneri opere mediche e fisiche, tom. 2. fol. Venet.

Nuove ed erudite osservazioni, storiche, mediche, e naturali del Dottor Girolamo Caspari, medico primario di Feltre, 8vo, Venet.

Saggio di medicina teorico-practica di Carlo Gianello, 8vo, in Venezia.

Observationes de febribus praecipue intermittentibus, et ex earum genere continuis, deque earum periculis ac reversionibus praenoscendis et praecavendis per medelam tempestivam, efficacem, adaequatam, candide et perspicue propositam. Autore Paulo Gottlieb Werlhof, M. D. 4to, Hanover.

A discourse concerning fevers, in two letters to a young physician. The second edition, 8vo, London

Frederici Hoffmanni medicinae rationalis systematicae, tomi 4ti pars 2da, 4to, Halæ Magdeburg.

XXXI. *An Account of the most remarkable improvements and discoveries in Physic made or proposed since the beginning of the Year 1733.*

DR Boerhaave, professor of physic at Leyden, has communicated to the Royal Society at London, several accurate and very laborious experiments concerning mercury. The result of them may be summed up in these few articles.

1. Quick-

1. Quick-silver, however well purified, by passing through leather, washing, or distillations, repeated ever so oft, yields always a soft black powder of a sharp brassy taste, when long exposed to violent conqassation, or to a degree of heat about twice as great as that of animals.

2. Heat, near as strong as what is necessary for distilling quick-silver, changes the greater part of mercury, if not all of it, into a heavy, shining, red, friable powder, of a very sharp, metallic, nauseous, penetrating taste, which one can scarce free his mouth of, that long and violently disorders the human body, and disposes it to excretions.

3. The fluid quick-silver remaining after this red powder is separated, is more fluid, and of less specific weight than common mercury.

4. All the black, and very near the whole red powder, can be brought into the former fluid state, by a more intense heat; and this revived quick-silver enjoys all the properties of common mercury, and serves as well for repeating the same experiments.

5. The small part of the red powder which is not revived can scarce be raised by the force of fire, and becomes of a dusky colour, swelling like a sponge, and vitrifies by the addition of some borax; but, when tried with lead, it flies off.

6. Quick-silver, when distilled by itself, or in water, requires a very strong heat to raise it; but, if it is previously reduced to a black powder, by amalgamizing it with one fourth part of lead, and then is put among vinegar,
the

the quick-silver rises before the vinegar boils, Philosoph. Transact. numb. 427. § 2.

In pag. 324. of vol. II. we mentioned quick-silver being the fashionable medicine at London; since which it has been given here at Edinburgh in several forms, different from those commonly in use before. Tho' several have taken an ounce or two of crude mercury each morning of several weeks, we know no instance of its increasing any of the sensible evacuations; but have been told, that some who used it thus had passed some of it with their urine, and that the hands of others under this medicine had gilded their snuff-boxes and heads of their canes.

Quick-silver rubbed violently with a double quantity of crabs-eyes, or of sugar candy, till it is extinguished, when it goes by the name of *mercurius alkalizatus*, or *Æthiops albus*, tho' it has been taken by some without any sensible effect, yet we have had the experience of a very small quantity of it having raised a high salivation to others.

Mercury, extinguished in prunells, has operated much in the same way with the *Æthiops albus*.

Quick-silver dissolved, by rubbing it strongly with any chemical oil, or with venice turpentine, has been given to the quantity of a scrupule, half a drachm, or two scrupules in a day. Such pills keep some peoples belly open, others they purge, and a gentle ptyalism has sometimes been occasioned; nay, a high salivation has been raised by them.

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cutaneous diseases, and obstinate glandular swellings, when care has been taken by doses adapted to the patients, and the proper use of gentle opiats, or purgatives to prevent its running off by stool, or its affecting the salivary glands too much.

An imitation of Belloste's pills has also been tried; these are made by extinguishing quicksilver in resin of guajac, then forming into pills, with a chemical oil, or with *ol. tartar.* and afterwards crusting each of them over with the extract of rhubarb. These purge gently. We have not heard that they increased the excretion of saliva.

Crude antimony is recommended in palsies, pains, and numbness that come on after a salivation, and is said to have cured several who were paralytic from other causes. The method of giving it, is to begin with three grains, increasing the dose with three grains every day, till the patient takes half a drachm at once; after which the dose is diminished three grains every day, till it comes down to the quantity of the first dose. *Commerc. Literar. Norimberg. 1733. hebdom. 2. § 2.*

The French physicians are at present much divided in their opinions concerning the medicine so famous at Paris in 1719 and 1720, under the name of the powder of the *Chartreux*, which is now generally named *kermes mineral*. It is the sulphur of antimony prepared, by pouring a dissolved fixt alkali salt; for example, *ol. tartari per deliquium* upon the powder of antimony; and, after digesting them in a sand heat for a day, to make the liquor boil two hours, when

when it becomes of a very red colour; and being poured off into another vessel, lets fall a red powder, which is freed of the alkali salt, by washing it frequently with water; after which it is dried and kept for use. Half a grain, or a grain of this powder, given every three or four hours, produces no violent effects; but, by increasing the dose, it may be made to vomit, purge, and sweat. Some commend it in their theses and pamphlets, as the most universal solvent and deobstruent, assuring us, that it almost infallibly cures pleurifies, peripneumonies, asthmas, catarrhs, anginæ, small-pox, and many other diseases. Others, on the contrary, are as positive, that it heats and thickens the blood, thereby increasing obstructions, and is particularly hurtful in all inflammatory diseases. Unluckily both parties appeal to experience, and mention examples to support what they argue for.

By the information we have received of the effects of the medicine upon which the great run is now at London, we can imagine it to be no other than a preparation of antimony, and that no milder one than the butter of antimony; than which there is scarce known a more violent destroyer of all animal substances, being composed of the reguline or metallic part of that mineral corroded by the concentrated acid of spirit of nitre, or of sea-salt.

Dr Boeli at Brunswick recommends the powder of the root of the valerian taken into the nose by way of snuff, as an excellent restorer of weak sight, and mentions some examples of his success with it. As he orders it, there may

be some doubt on what the success depends. His receipt is, *R. Rad. valerian. fol. tobac. a. drach. ii. fiat pulv. subtilissim. Adde ol. destill. lavendul. Majoran. a. gutt. iii. M. Aët. physico-medic. acad. natur. tom. III. obs. 125.*

Sir Hans Sloan, president of the royal society, informs us of four children, who, after eating the seeds of the *hyosciamus niger* C. B. or common henbane, were seized with great thirst, swimmings of the head, dimness of sight, ravings, and profound sleep; which last continued two days and nights in one of them. He says the delirium occasioned by these seeds, differs from the common, and in some measure agrees with that produced by the dutroa, a species of the stramonium, and by the bangué of East India, a sort of hemp.

He cured all these children by bleeding, blistering in several places, and purging afterwards, with a medicine composed of *elect. lenitiv. ol. amygd. d. flor. sulph.* and *syrup. pæon.* which operated both by vomit and stool.

The same gentleman also tells us of a quack, who cured the toothach, by conveying the smok of burning henbane seeds, by means of a funnel, into the hollow tooth. *Philosoph. Transact. num. 429. § 1.*

Dr Short, in his history of the mineral waters of Yorkshire, Derbyshire, and Lincolnshire, has classed them into the warm, purging chalybeat, diuretic chalybeat, purging and plain sulphur waters.

He found the warm waters to be impregnated with a mineral steam, vapour, or spirit, containing a most subtile and impalpable sulphur, with

a calcarious earth, and some nitre and sea-salt. Of those he mentions, that of Buxton seems to be the principal, the heat of which is equal in frosty weather to common river water with which two fifths of boiling water has been immediately mixed.

The purging chalybeat waters contain a mineral spirit, sulphur, vitriol, nitre, and sea salt, with a calcarious earth, of which some particles are attracted by the loadstone. Of these, Scarborough spaw is now in greatest reputation; the principal salt of which is described and painted by our author, as consisting of long crystals made up of six sides, which all concur at each extremity in forming a diamond point, and he calls it *nitre*.

The diuretic chalybeat waters consist of much the same principles with the former class, only the salts are in less proportion. Of these there are great numbers in Yorkshire.

The sulphur waters, besides sulphur, contain also marine salt and nitre, or nitre only and earth. Of these the strongest is Harrowgate-well.

Dr Shaw, in his treatise on Scarborough waters, previously lays down a great many rules to be observed in the investigation of all mineral waters, whereby we may be directed in our inquiries, and may be cautioned against drawing conclusions too hastily, which deserve to be seriously considered by all employed in physic.

The contents of the purging spaw, which is what is chiefly used, are much the same in Dr Shaw's account, as what we mentioned from

Dr Short; only Dr Shaw has described the first salt, as consisting only of four sides, of which two are broad, and two are narrow; the two broad ones, which are opposite to each other, are extended into a sharp edge at one extremity of each crystal, the narrow sides not shooting out so far; and the reverse is observed at the other extremity of the crystal. He affirms this salt to be *sui generis*, and different from all the natural or artificial salts yet known.

This water proves a gentle cooling purgative; but, as the salt is in a small quantity, the operation of the water is often required to be assisted by some of the salt prepared by the apothecaries.

Dr Waltherus, professor at Leipfick, after comparing the muscles of the human body, with the descriptions of them published by several authors, particularly by Mr Winslow (in his *exposition anatomique*) has made several accurate remarks on them, which may serve as a supplement to the *anatomie teneriorum musculorum repetita*, which he published formerly; but, as they will not admit of an abridgment, we must refer to the observations themselves, which are inserted in *Nov. adl. erudit. mens. Jun. 1733.*

Dr Nichols, lecturer of anatomy at Oxford, has, in his *Compend. anatom.* contradicted the common received doctrine of the motion of the heart, and of the circulation of the blood, both in adults and fœtuses.

Prælect. XV. "The circulation of the blood depends, (says he), on six motions, 1. Of the right auricle. 2. Right ventricle. 3. Pulmonary artery. 4. Left auricle. 5. Left ventricle;

"ventricle; and, 6. Of the aorta. Of these
 "the 1st, 3d, 5th are synchronous, or act
 "at the same time, as the 2d, 4th, 6th like-
 "wise do; but the 1st, 3d, 5th, are asyn-
 "chronous, or act at a different time from what
 "the 2d, 4th, 6th do; and therefore

The 2 auricles	} alternately	{ are relaxed,
The 2 ventricles		{ are con-
The 2 arteries		{ tracted.

Prælect XXIV. Our author has the follow-
 ing propositions concerning the circulation of
 the blood in foetuses.

"1. The blood of the ascending cava is fitter
 "for nutrition, muscular motion, and the sub-
 "tile secretions, than the blood that is carried
 "to the heart by the descending cava. For the
 "former consisting partly of blood lately tritu-
 "rated in the lungs of the mother, partly of
 "blood returning from the *vena porta*, and de-
 "fecated in the liver, with the blood brought
 "back from the iliac and emulgent vessels,
 "may be looked on as arterious blood; the lat-
 "ter, on the contrary, being deprived of many of
 "its more ubtile parts, bestowed on the nou-
 "rishment of the fibres, or in the secretions of
 "the brain, is altogether venous, and as it were
 "weak and poor, (*effætus*.)

"2. The ascending and descending aorta are
 "dilated and contracted at different times, or
 "have asynchronous motions.

"3 The blood of the ascending cava is push-
 "ed to the heart, at the time when the right
 "auricle is contracted, and the left auricle is
 "relaxed; and therefore it will not pass into the

“ right auricle, and from that into the left ; but
 “ must go immediately from the cava into the
 “ left auricle

“ 4. The blood which is sent from the left
 “ auricle, into the left ventricle, consisting
 “ mostly of the blood of the ascending cava, is
 “ wholly distributed to the heart and branches
 “ of the ascending aorta.

“ 5. The blood which flows from the descend-
 “ ing cava into the heart, passes partly through
 “ the lungs into the left auricle, to be mixed
 “ with the blood of the ascending cava, partly
 “ passes into the descending aorta, not to be
 “ mixed with the blood of the ascending artery ;
 “ that the blood which is returned to the mo-
 “ ther may be venous, weak, and poor, (*effætus.*)

“ 6. The *canalis arteriosus* being shut by re-
 “ spiration, the descending artery acquires a
 “ motion synchronous to that of the ascending
 “ artery ; and the blood of the ascending cava is
 “ sent to the heart, at the time when the left
 “ auricle is contracted, and the right auricle is
 “ relaxed ; and therefore is wholly poured into
 “ the right ventricle, along with the blood of
 “ the descending cava.

“ 7. The contents of the abdomen being pres-
 “ sed by respiration, the umbilical arteries, um-
 “ bilical vein, and the *ductus venosus* are soon
 “ shut up.

“ 8. The usual crying of new-born infants
 “ contributes much to the distension of the
 “ lungs, and breaking down the particles of
 “ the blood.

The Doctor next explains a figure he has
 caused to be drawn of the two auricles of the
 heart

heart of a foetus to shew the canals, by one of which the *vena cava ascendens* opens into the right, and by the other into the left auricle, at each of which he paints a valve.

He illustrates afterwards his scheme of the circulation of the blood in a foetus by another figure, in explaining which he assumes the second and third of the preceeding propositions as demonstrated.

Dr Nichols's opinion concerning the circulation of the blood *in natis et non natis*, in born and unborn animals, is so different from what has prevailed since Harvey's time, that we cannot but wish he had been more explicit, and would add the experiments or other proofs that can be brought to support his doctrine. With a view to be informed, and to induce perhaps the Doctor to explain himself more fully, we shall propose one question which naturally offers itself, upon looking at his scheme of the circulation in a foetus. What preserves the form of canals to F, (the passage from the *cava ascendens* into the right auricle), and to N, (the part of the aorta between the rise of the left subclavian artery, and the insertion of the *canalis arteriosus*), seeing, by the explication of the scheme, there are no liquors pass through them?

The cataract is generally now agreed to be for most part the crystalline humour rendered opaque, very few instances being brought of any membrane lodged in the posterior chamber of the aqueous humour, and these have been suspected to be no other than the membrane of the crystalline separated. To confirm more the doctrine of the crystalline being the part affected

fect in the cataract, Dr Scheuchzer, physician at Zurich in Switzerland, communicates the dissection of two cataractous eyes, on one of which the operation had been performed with success. In the eye which had not been couched, the crystalline, which adhered to the circumference of the uvea, was of the colour of whitish pearl, but less bright. In the other eye, the crystalline was less, harder, inclined to a yellow colour, and depressed below the uvea. A cloud was observed in the part of the vitreous humour through which the needle had passed. *Act. physico-medic. acad. N. C. tom. III. observ. 36.*

The cases in midwifery, mentioned by Mr Giffard, may be reduced to these two, 1. That the head of the child is fallen down among the bones of the mother's pelvis, and does not advance with her throws, and cannot be pushed back into the womb. 2. That a wrong posture of the child, the flooding or weakness of the mother, make it necessary to bring the child away by the feet.

In the former of these two cases, he made use of an extractor, which is so far different from that described in Art. XX. of this volume, that the blades of it consisted each of an oval ring bended, instead of an entire piece of thin steel. Dr Hody, the editor of Mr Giffard's book, publishes with it an improvement made on the extractor by Mr Freke surgeon to St Bartholomew's hospital, which seems to consist in one of the handles having a joint in the middle of it, and its extremity being a sharp hook or crotchet, which has a moveable flap to cover it, when it is not employed as a hook to operate with.

with. Mr Giffard observed sometimes, that the *os uteri* gripped the child's head so firmly, that it could not be brought away with the extractor, till with his fingers he had dilated the ring which the womb made. As soon as he could catch hold of the head with his hands, he laid aside the instrument. If, after the head was born, the child stuck at the shoulders, he endeavoured to make more way for them, by bringing out first one arm, and then the other; or, if that could not be done, he put a crooked finger into each arm-pit of the child, and so extracted it.

In the cases where there is a necessity of bringing the child away by the feet, he dilates the *os tinea* by spreading his fingers, pushes back what parts are in his way, or slides his hand along them to search for a leg: After he has brought this out, and tied a ligature upon it, he searches for the other, but is not anxious about finding it, if it is bended upwards on the child's belly, or the passage is wide enough by the woman's having formerly born children; for the leg he has brought out is sufficient for bringing down the buttocks, till he can put his crooked finger, or the handle of one blade of the extractor armed with cotton in the groin, or a fillet put round the thigh; with any of which and the ligature on the other leg, he extracts the buttocks. When laying hold of the child's body, he turns it, if there is occasion for it, as he says there commonly is in such a case; because the child has sunk down in the womb without turning, so that its face is forward. When the shoulders are brought out, he

he brings down the arms to make more room, and then clapping one hand flat upon the breast of the child to support it, he lays the other on the shoulders, and pulls. If the *os internum* has contracted round the neck, he dilates it: If the head sticks notwithstanding this, he advances the lower hand, till he can put a finger or two into the child's mouth; and then pressing on the lower jaw, he draws with both hands, and seldom misses to bring the child away.

Our author took always care to put back the umbilical rope when it falls out before birth, and to disengage it from the child's neck or body, or to cut it when it is twisted round any of these parts, to prevent the placenta being pulled away, or the circulation being stopped in it.

He always separated the placenta with his fingers, and did not pull it away by the navel-string, and took care to free the womb afterwards of clotted blood and membranes, which, when left, occasions pains and flooding. He is of opinion, no time should be delayed in doing this after delivery, because the womb contracts very fast.

Clysters to provoke too languid throws, an opiate to put away false pains, and to bring on the true ones, and a cordial draught after delivery when the woman was low, were all the medicines he used.

Mr Chapman, in his essay on the improvement of midwifery, p. 7. affirms, that he never did observe the lateral pointing of the womb, so much insisted on by Deventer, but has observed it turned forwards or backwards.

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P. 10. He condemns greatly the use of the crotchet or hook in the delivery of women, unless when there is an entire certainty of the child being dead; which only can be determined by a concurrence of all the symptoms mentioned by authors, and not by one or two of them.

P. 12. He condemns the make of the extractors he has seen others employ, but does not describe his own, nor his manner of slipping a fillet over the child's head.

P. 67. The most effectual remedy in flooding is, according to him, to cover the patient's body with clothes dipped in oxycrate, repeating them as they grow warm, and giving cool acid liquors to drink.

P. 117. After a hard labour, and where there has been a necessity of using some violence, "the woman (says Mr Chapman) is to be treated as one bruised by a fall. Here, as a thing of the greatest service, I would recommend wrapping of the body round with sheep's skin hastily flea'd off, and applied as warm as possible: I have, for many years past, had a happy experience of this, and wish I had come sooner to the knowledge of it than I did, as having always made use of it with success." This method is recommended by Guillemeau.

Dr Schulze, professor of medicine at Hall, contradicts the common opinion concerning the umbilical vessels, in two dissertations he published, to be defended by his scholars. He endeavours to prove in the first, That the navel is not formed by the ligature which the midwives make,

make, or by animals gnawing the navel string of their young with their teeth, but by nature; and that the umbilical vessels separate spontaneously, or with very little force, from the interior surface of the skin, in the same manner as we see the shrivelled navel-string fall off from the exterior surface. After which the umbilical vessels within a child's and other young creature's body contract and shrivel, their extremity by which they adhered to the navel becoming black and pointed, as if they had been burnt, and at last disappear altogether, without leaving any vestige that can be observed in the adult; for, says he, what are commonly described and painted, as these vessels changed into ligaments, are no other than the sheaths in which they were formerly contained.

It is easy to see what conclusion he draws from this doctrine in his second dissertation, viz. That it is unnecessary to make any ligature upon the navel-string after birth; but, as he judges the ligature to be innocent, he does not insist upon its being omitted.

In confirmation of Dr Schulze's doctrine, Dr Eller, physician at Berlin, relates several instances of the navel-strings of children being left untied after they were cut, without being attended with any hæmorrhage, or other bad consequence. *Commerc. Norimberg.* 1733, hebdom. 48, §. 2.

Dr Trew of Norimberg, having carefully examined the state of the umbilical vessels of subjects of different ages, observes that the shrivelling is not peculiar to these parts, several other parts

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parts being changed in the same way, he could discover no mechanism by which a hæmorrhage should be prevented without a ligature, when the umbilical vessels are cut; and concludes, that, seeing there are examples of hæmorrhagies from neglecting to tie the navel-string, it would be very unsafe to forbear the use of the ligature, *Ibid. hebd. 49. § 1. & hebd. 50. § 1.*

Dr Alexander Stewart, physician to the queen of England, formerly * communicated an observation of a serjeant of the horse-guards, whose gall-bladder was wounded without any other bowel being much hurt, and whose symptoms, in the seven days he lived after being wounded, were a great distension of his belly, without ructus or flatus upwards or downwards, or borborygmi; no passage by stool, and very little urine, notwithstanding purgatives and clysters were given him, and that he took what was thought a sufficient quantity of drink and liquid food; neither had he any sound sleep, but only short slumbers, tho' he took opiates. There was no sign of fever, his pulse continuing in a natural state till the day before he died, when it intermitted. The wound in the teguments never supplicated well.

After his death, the guts were found greatly distended, the gall-bladder was almost quite empty, and a great quantity of bile was lodged in the cavity of the abdomen.

The doctor accounted for all these appearances, from the defect of bile within the intestines,

VOL. III.

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testines,

* Philosoph. transact. num. 414. § 2.

testines, to stimulate them to a due contraction, whereby they might resist the elastic air, or might push the food or drink into the lacteals, or might expel their contents.—Their depriving him of a due recruit of chyle, occasioned his want of sleep, diminished the secretion of urine, and prevented a suppuration in the wound.—The vessels being emptied by the excretions that were not compensated by a new chyle, while the more acrid particles of the blood were carried off in the secretion of bile, without any of them returning to the blood again; and therefore, the vessels being neither stretched nor irritated, there could be no fever.—Since there was a constant waste of this man's liquors without any supply from the food, the doctor concludes him to have died famished.

This account of the symptoms, however ingenious, did not fully satisfy some people, whose difficulties our author now endeavours to remove.

To those Gentlemen who remark, that the gall (especially in such large quantity as in the history) when applied to the exterior surface of the guts, might have irritated some particular parts of them to a spasmodic constriction, which would have occasioned the distension of the intermediate parts, and the other symptoms, as well as the causes assigned above, the doctor replies, That nerves only exert their action at their extremities, where they are divested of their involving membranes; and therefore, in the case before us, the bile could not have given pain, nor excited a contraction in the muscular

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cular fibres of the guts. 2. The irritation of the exterior membrane of the guts would have no effect on the muscular coat, because the former has its nerves from a source different from those of the latter. 3. The gall, being equally diffused over the surface of the guts, must have made an equal contraction of their fibres every where, which was not the case.

In explaining how a fresh recruit of chyle should be a cause of sleep, Dr Stewart is obliged to examine how and by what sleep is produced.

“ I believe (says he) it will hardly be denied, “ that the cause of sleep in general is a want of “ a sufficient quantity of animal spirits,” [for the existence of which he appeals to the arguments and experiments mentioned p. 326. of Vol. II.] “ for the use and exercise of the animal functions; therefore, whatever prevents their recruit, hinders or impedes their secretion, absorbs or fetters them when produced; and “ whatever exhausts and evaporates them, by “ occasioning a paucity of spirits, will, in a “ healthy person, produce a listlessness, laziness, “ a tendency to sleep, or sleep itself, in proportion to that paucity of the remaining spirits.”

To illustrate this doctrine, he considers the remoter causes of sleep, which he reduces to four. 1. Exercise. 2. A too plentiful meal. 3. Drunkenness. 4. Narcotics. And then proceeds to shew how these produce such effects.

Exercise wastes all the fluids, and particularly the animal spirits.—The great quantity of chyle, after a full meal, makes all the fluids

of a thicker consistence, and absorbs the animal spirits.——Fermented liquors and ardent spirits, being observed to diminish remarkably the serous secretions, may therefore be reasonably concluded to fetter the finest fluid, which is the animal spirits, and to hinder it to be separated from the other liquors,——Soporifics act much in the same manner.

According to our author, in sleep, a sufficient quantity of spirits still remains for actuating the organs of the vital and natural functions, to which they are determined by more powerful causes, such is the impetuous blood in the heart, gravitating air in the lungs, digesting food in the stomach, &c. than there are to determine them to the organs of the animal functions. Hence, awaking is owing to a quantity of spirits so much greater than what is required for the vital and natural functions, that they must excite the animal likewise.

The last proposition which the Doctor undertakes to prove is, "That pus, being a gross secretion, is the product of the chyle, and not of the blood or serum; for (says he) I think that it would not be difficult to prove, that all the gross secretions are from the chyle." *Philosoph. Transact. numb. 427. § 2.*

Mr Evan Davis communicates to the Royal society the histories of several children inoculated with the small pox at Haverford-West in Pembrokehire, while the measles were epidemic there in February and March, after the small pox had been very mortal through the winter. Every one of them who were infected thus with the variolous matter, only became feverish in the

the seventh or eighth day after the inoculation, and the measles appeared soon after. On the twelfth day, they were again attacked with a fever, and on the fourteenth the small pox, of a mild kind, were seen; *Ibid. numb 429. § 9.*

Dr Lobb, in his rational method of curing fevers, after having examined the nature of the fluids and solids of the human body, things necessary to health, (such as, the air, aliments, secretions, and evacuations), the causes of diseases, and particularly of fevers, concludes, p. 194, That, whatever may be the productive causes of fevers, the state of the fluids in people under them must be one or other of these following:

1. That the animal fluids are too thick, that is, they are viscous or glutinous, which seems to be the case in all inflammatory fevers. Or,

2 That they have particles too bulky for an easy circulation and excretion, till they are comminuted. Or,

3. That the animal fluids are too thin, and the natural union of their component particles is more or less dissolved, and the globules of the blood and lymph more or less broken: Which appears to be the case in putrid, malignant, and many pestilential fevers, and in all fevers attended with colliquative evacuations. Or,

4. That the fluids have acrimonious and corroding particles mixed with them, which is the case in all fevers attended with ulcerations from an internal cause.

While the fluids are thus affected, the solids are rigid or too dry; or they are too lax.

It is impossible for us to mention, in the narrow bounds we are confined to, all our author's curative indications taken from the foregoing doctrine; we shall therefore do no more than remark, that he allows of blood-letting only to patients who have a sanguine plethora, for the knowledge of which he refers to his treatise of the small pox, where, as we mentioned in Vol. I. p. 286 he confines it to so many circumstances, that it will seldom be met with; and in those patients to whom letting of blood is proper, "the quantity of blood (says he, p. 208.) "that may prudently be taken away at once, "I think, generally speaking, should not exceed "six or eight ounces; because we cannot know "to an ounce or two the exceeding quantity, and "because, if it should afterwards appear, that "too little has been taken away, the remedy is "easy, by repeating phlebotomy; but, if too "much has been drawn off, it is not easy to redress the inconveniencies that may attend it." Afterwards, in considering the effects of blood-letting, Dr Lobb deduces from Mr Hales's experiments, that taking away six ounces and two drachms of blood from a man weighing 160 *lib.* may diminish the vital strength, or the force of the action of the heart and arteries, about one tenth part. The loss of twelve ounces and a half of blood will diminish his vital strength above one sixth part, which, says he, is a great diminution. Eighteen ounces and six drachms will diminish it above one fourth, which, he adds, is a vast diminution, and to be avoided, unless some extreme necessity should require the taking

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king of it. Thirty-seven ounces and a half of blood taken away, diminish the vital strength of such a man near one half, a diminution which surely ought to be dreaded. Fifty ounces may diminish it near six sevenths; the consequences of which may, says our author, be easily apprehended. From the whole, we think it may be concluded, that the doctor is not very fond of taking blood, especially in a large quantity.

XXXII. *A List of Medical Books published since the Beginning of the Year 1732.*

A *Lbun Bavariæ Patræ, seu catalogus celebriorum aliquot medicorum, qui suis in Bavaria scriptis medicinam exornarunt, ab anno 1450, quo Boica schola fundata quidem, at primum anno 1472 publicata fuit, in hodiernam usque lucem.* Studio Franc. Jos. Grienwaldt, M. D. 8vo, Monachii 1733.

The treasury of drugs unlocked; by Jo. Jacob Berlu, of London, merchant in drugs. The second edition, 12mo, London 1734.

A treatise on the force and energy of crude mercury, by Thomas Harris, surgeon, 8vo, London 1734.

A critical dissertation on the manner of the preparation of mercurial medicines, by T. K. M. D. 8vo, London 1734.

Hampstead-wells; or directions for the drinking those waters; with an appendix relating to the original of springs in general; with some experiments on the Hampstead-waters, and histories

stories of the cures, by John Soame, M. D. 8vo, London 1634.

An inquiry into the contents, virtues, and uses of the Scarborough spaw waters, by Peter Shaw, M. D. 8vo, 1734.

An inquiry into the nature and principles of the spaw waters, by Charles Perry, M. D. London 1734.

Toxicologia pathologico-medica, sive de venenis, lib. III. Autore Christiano Godofr. Stentzelio, M. D. 4to, Vitemberg 1733.

Chemical lectures publicly read at London in the years 1731 and 1732, and since at Scarborough in 1733, for the improvement of arts, trades, and natural philosophy, by Peter Shaw physician at Scarborough, 8vo, London 1734.

Traité de chemie, contenant la maniere de preparer les remedes qui sont le plus en usage dans la pratique de la medicine. Par M. Maluin Docteur regent de la facultée de medicine de Paris, 12mo, Paris 1734.

Osteographia; or, the anatomy of the bones, by William Cheselden surgeon to her Majesty, &c. fol. London 1733.

The anatomy of the human bones, with an account of muscular motion, and the circulation of the blood; also of digestion and nutrition, with a description of the four senses, illustrated with variety of copper-plates. To which is added, a short and easy method of discovering the virtues of plants, in curing the diseases of the human body; by George Thomson, M. A. 8vo, London 1734.

Spiramina, or respiration reviewed; being chiefly the argument of that great philosopher by

by fire, *Jo. Bapt. van Helmont*; discovering certain uses of the lungs not commonly observed, and asserting that they have not that alternate motion that is generally ascribed to them; but that in a sound man they are porous, pervious to the air, and constantly at rest, by *M. J.* 8vo, London 1733.

Compendium Anatomicum, ea omnia complectens quæ ad cognitam humani corporis œconomiam spectant, &c. constructum, à F. Nichols *è Coll. Oxon. M. D. Præl. Anat. Oxon. &c.* 4to, Londoni 1733.

Physical essays on the parts of the human body, and animal œconomy, 8vo, London 1734.

Anatomie chirurgicale de Palfyn, revue, corrigée, et augmentée, accompagnée de notes dans le premier volume, et refondue dans le second, par M. B. Boudon, docteur en médecine. On y a joint les observations anatomiques et chirurgicales de Mr Ruysch, traduites du Latin, et celles de M. Brisseau, en 2 vol. 8vo, Paris 1734.

The navy surgeon; or a practical system of surgery, by *John Atkins* surgeon, 12mo, London 1734.

Two hundred and twenty five cases in midwifery, which for the most part were attended with a great deal of danger and difficulty. Written by the late *Mr William Giffard*, surgeon and man-midwife, published by *Edward Hody*, M. D. 8vo, London 1733.

The art of nursing, the second edition, 8vo, London 1733.

An essay concerning blood-letting, by *R. Butler*, M. D. 8vo, London 1734.

Suite

Suite des maladies chroniques, ou l'on traite celles qui arrivent à l'oeil, et des remèdes les plus convenables pour les guerir sans operation manuelle, par P. V. Dubois, ancien Prevôt et garde des maitres chirurgiens de Paris, tom. V. 12mo, Paris 1733.

Nouvelles classes des maladies dans un ordre semblable à celui des botanistes, comprenant les gendres, et les especes de toutes les maladies, avec leurs signes et leurs indications; par Sauvage de la Croix, docteur en medicine, 12mo, à Avignon 1733.

A compleat treatise of the stone and gravel, by N. Robinson, M. D. the third edition, 8vo, London 1734.

Meditationes theoretico-practicae de furore haemorrhoidum internarum, methodicè conscriptæ à Justo Arnoldo Gulich, M. D. editio altera, 8vo, Lugd. Bat. 1733.

An essay on the gout, by T. Bennet, M. D. 8vo, London 1734.

Rational methods of curing fevers, deduced from the structure and oeconomy of human bodies, and the different states of the solids and fluids, under the different classes of fevers; by Theophilus Lobb, M. D. 8vo, London 1734.

The generous phyfician, or medicine made easy; by Dr Colbatch, 8vo, London 1733.

Frederici Hoffmanni consultationum et responsorum medicinalium centuria prima, completens morbos capitis et pectoris, tom. I. 4to, Halæ Magdeburg 1734.

Michaelis Alberti Acad. Frid. Prof. Med. &c. ulterior continuatio, aut tomus 3tius jurisprudentiae medicae, 4to, Schneberg. 1733.

Caroli

Caroli Musitani jatrias prof. opera omnia.
Edit. 2da, 2 vol. fol. Lugdun. 1733.

The Philosophical Transactions (from the year 1720, to the year 1732) abridged and disposed under general heads, by Mr Reid and John Gray, F. R. S. 2 vol. 4to, London 1733.

The Philosophical Transactions (from the year 1719, to the year 1733) abridged and disposed under general heads, by Mr John Eames and Mr John Martyn, F. R. S. 2 vol. 4to, London 1734.

Acta physico-medica academïæ Cæsareæ Leopoldino-Carolinæ naturæ curiosorum, exhibentia cephemerides sive observationes, historias, et experimenta a celeberrimis Germaniæ et exterarum regionum viris habita et communicata, singulari studio collecta. Volumen tertium, 4to, Norimberg, 1733.

Philosophical Transactions for the year 1733, 4to, London.

Commercium literarium Norimbergense anni 1733. semestr. 2. 4to, Norimberg.

Dissertationes medicæ, quas, ex auctoritate amplissimi senatus academici Edinburgensis, et nobilissimæ in eadem academia facultatis medicæ decreto, pro gradu doctoratus, summisque in medicina honoribus et privilegiis rite ac legitime consequendis, examini subjecerunt.

Joannes Lindefay Scotus, *De calore.*

Jacobus Grieve Scoto-Britannus, *De morbis humorum oculi.*

Carolus Ayton-Douglas Scot.-Brit. *De exercitationum in medicina usu.*

Joannes Arnot Scotus, *De abortu.*

Henricus Tonge Anglus, *De noxis ex cibi et potionis abusu oriundis.*

XXXIII. *Books proposed, and other medical News.*

LR Boerhaave, professor of medicine at Leyden, concludes the account of his experiments on mercury, with a promise, conditional indeed, but which probably the importunate requests of the learned will obtain, of publishing his experiments and remarks concerning the extraction of mercury out of metals, the action of mercury on metals, and concerning metals.

The new edition of the Edinburgh Dispensatory is now delivered to the printers, and will speedily be published.

Doct^r Christ. Jac. Trew of Norimberg has dispersed proposals for publishing a full, faithful, and distinct delineation and explication of all the parts of the human body. He does not propose to publish this whole work at once, but divided into sections. The osteology seems to be ready for the press; for the proposals inform us, That the bones of a young man are delineated in twelve tables in folio; the thirteenth plate represents the bones of the female which differ from the male. The conjunctions of the bones are to be explained in a plate larger than the others. The explication of these plates in high Dutch will be twelve sheet; and he promises this shall be translated into Latin or French, and printed, if any desire it.

Dr Thomas Simson professor of medicine in the university of St Andrew's designs to publish soon, a second edition of his system of the womb.

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The theoretical part of which he is to enlarge considerably, and is to add a practical part, wherein he is to treat of the diseases of the womb, and their cure.

Dr Lobb tells us, in his rational methods of curing fevers, That he has a treatise on the particular species of fevers near ready for the press, which he designs to publish soon, that he may complete his account of fevers.

Dr Michelotti, physician at Venice, has promised soon a treatise on the small pox.

Dr Grubert, physician at Brussels, is said to be about to publish a Medical Dictionary, in which there are to be several discoveries.

The subject proposed by the chirurgical academy at Paris, for gaining the prize of 1734, is, "To determine, in each kind of chirurgical disease, the cases where it is fit to dress frequently, and those where it is fit to dress seldom."

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